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## THE DEFINITION OF PSYCHOSOMATIC DISORDER \*

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ANY attempt to add to the literature of psychosomatic medicine must have a very good excuse. Mine is that so much of this literature consists of discussions as to whether this or that disorder is psychosomatic, or whether any disorder is psychosomatic, and so little of it is devoted to deciding what sort of a thing a psychosomatic disorder is. Much of the discussion arises in one of two ways. Either the participants have different views on the mind-body relationship, and therefore cannot agree for metaphysical reasons; or else they have each treated successfully a case of what is to all appearances the same disorder, but one has done so by physical means and the other by psychotherapy. I hope to show that there is one definition of psychosomatic disorders which avoids metaphysical questions and which takes into account the successful treatment of such disorders by both physical and psychotherapeutic means. Before offering this definition I shall consider how the fashionable definitions are related to certain views on the mind-body question, and what difficulties arise from these relationships. If, after all this, my suggested definition seems too obvious and too much of an anticlimax, I apologise in advance.

I must make it clear that I intend no criticism of the methods employed by any reputable section of the medical profession in treating this kind of disorder, nor are any of my arguments directed to show that one type of treatment is likely to be more effective than another. These are questions which I am not qualified to discuss. If I am committing trespass, it is trespass without damage. But I do not think it is trespass, since it seems to me that a right of way has been created. There has been a good deal of philosophising about medicine by physicians, surgeons, and psychiatrists whose medical

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qualifications are more obvious than their philosophical ones, and they cannot very well complain if someone in the opposite position asks to be allowed a turn, even if the results seem as bizarre or banal to them as theirs to the philosophers.

### I *The Classification of Disorders*

Present-day medicine seems to me to have two main ways of classifying disorders—by symptom and by cause. There are the disorders of the skin, of the eyes, of the respiratory system, of the digestive system, and so on. Not only are most disorders named after their most striking symptom (smallpox, for instance), but also it is on this sort of classification that the subdivision of the medical profession into ‘specialities’ is based. If my skin is inflamed or itchy I consult a dermatologist; if my heart troubles me I go to a cardiologist. The other method of classification—by causes—is a more recent development, as might be expected; it is the product of a more highly developed stage of the technique. We now say that some disorders are infective, by which we mean that they are communicated by one person (or animal) to another by the transmission of a parasite, bacterium, or virus. Other disorders are ‘nutritional’; they result, that is, from some deficiency or excess in the diet. The two kinds of classification are not rivals; they do not seem to be competing for supremacy. If one doctor says that you have something wrong with your eyes, and another says that you are suffering from a localised infection, they are not contradicting one another. Indeed, the names of some disorders (for example, bacterial endocarditis) reflect both methods of classification.

Side by side, however, with this subdivision of disorders into symptoms and causes is our subdivision of people into mind and body. The things that happen to them happen either to their body or to their mind, according to our everyday mode of thinking. And when they suffer from disorders both symptoms and causes can be either bodily or mental, psychic or somatic. Whether this dichotomy is sound or not—a question to which I shall return—it is still widespread, and the result is a double dichotomy in our classification of human ills. This double dichotomy means that it is logically possible for human disorders to fall into nine categories:

- (A) Somatic symptoms with somatic causes;
- (B) Somatic symptoms with some somatic and some psychic causes;



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- (C) Somatic and psychic symptoms with somatic causes ;
- (D) Somatic and psychic symptoms with somatic and psychic causes ;
- (E) Somatic symptoms with psychic causes ;
- (F) Somatic and psychic symptoms with psychic causes ;
- (G) Psychic symptoms with somatic causes ;
- (H) Psychic symptoms with somatic and psychic causes ;
- (I) Psychic symptoms with psychic causes.

Although it is quite conceivable that no doctor has set out this nine-fold classification explicitly, either on paper or in his mind, two of these categories, the first and last, have been used for a very long time. If my skull is fractured by a falling tree, that is a somatic symptom from a somatic cause. If I see a dagger in the air when nobody else does, because I have made up my mind to kill my king, this is a psychic symptom from a psychic cause. Two of the intervening categories have also been used for some time : (C) and (G) have been recognised ever since men learned how to brew strong drink or love-potions.

It is the other five categories—those that link symptoms wholly or partly somatic with causes wholly or partly psychic—that medicine has been slow to recognise, for reasons which I shall shortly try to guess. These are the categories which are, I think, intended by the recently invented classification of certain disorders as ‘ psychosomatic ’. Here is Halliday’s definition :

A crude and preliminary definition of a psychosomatic affection would be : A bodily disorder whose nature can be appreciated only when emotional disturbances, that is, psychological happenings, are investigated in addition to physical disturbances, that is, somatic happenings. Or alternatively : A bodily disorder in which the application of the psychological approach provides information of high aetiological relevance.<sup>1</sup>

Clearly Halliday is talking here about my category (E) : disorders consisting of somatic symptoms and psychic causes ; but I have no doubt that he would also include under the heading ‘ psychosomatic ’ my categories (B), (D), and (F) (disorders whose symptoms included somatic ones and whose causes included psychic ones).<sup>2</sup>

<sup>1</sup> J. L. Halliday, *Psychosocial Medicine*, London, 1948, p. 45

<sup>2</sup> Someone who has been ticking off each of my nine categories as I have dealt with them may notice that I have not yet said anything about (H)—psychic symptoms with somatic and psychic causes. This is seldom distinguished in medical literature from (D) or (I) ; but it may amuse readers to think of examples.

I think, however, that the suspicions and criticisms which have been levelled at the classification 'psychosomatic' can be quite fairly discussed by assuming that when Halliday or Flanders Dunbar or their followers use the term they are thinking of my category (E) (somatic symptoms with psychic causes), although they would not exclude my categories (B), (D), and (F). I am sure that this assumption is not only true ninety-nine times in a hundred, but will also avoid complicating the metaphysical issues that have to be disposed of in the following discussion.

What are the reasons that underlie the arguments, doubts, and suspicions that have centred round this classification? They seem to me to be partly metaphysical and partly practical. The practical ones can be dealt with briefly as a start. The chief of these, I think, is the difficulty of observing psychic causes in comparison with the ease with which we observe somatic ones. If you are told that the cause of a disorder is a brain tumour or a damaged cardiac valve or a bacillus in the blood, you know that the microscope, the X-ray plate or at worst the autopsy will reveal some abnormality that you can see. But if you are told that the cause is anxiety, it cannot be demonstrated in quite so convincing a way. Sometimes observation of the patient for a reasonably short time will convince you of the presence of anxiety; sometimes the patient will even admit it. In other cases, on the other hand, the anxiety does not manifest itself recognisably until the patient has undergone treatment of a special kind. Altogether the observation of psychic causes is not nearly so easy or convincing.

The other practical difficulty is this. It is not uncommon to find two doctors disagreeing with each other on the question whether a particular disorder is psychosomatic, and to find that the reason is that one doctor has successfully treated it by psychotherapy while the other has succeeded with physical methods.<sup>1</sup> The former maintains that the disorder must be psychosomatic (that is, that the symptoms must have a psychic cause) because he has successfully treated the psychic cause by psychotherapy. The latter doubts this because his successful treatment was physical, and cannot therefore have touched any psychic cause. Both doctors may in fact have been equally successful (although the fear that the other's success may endanger their own theory often leads them to deny it). Both are making the understand-

<sup>1</sup> A logically similar disagreement can arise when one doctor is successful and the other unsuccessful in treating the same disorder by psychotherapy.



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able claim that they have put right something that was wrong with the patient—in one case with his mind, in the other with his endocrine, digestive, or cardiovascular system.

### 2 *Cause in Medicine*

In addition to these practical, or apparently practical, difficulties there are metaphysical doubts that seem to trouble doctors. One of these, I think, is the recollection that the notion of 'cause' has been pulled to pieces by philosophers until the very use of the word is now looked on as a mark of scientific naïvety. This has led to elaborate attempts to rehabilitate or eliminate it.<sup>1</sup> How legitimate is suspicion of this kind? It is true that the old-fashioned kind of causation, in which *A* was followed invariably and inevitably by *B*, is out of date in science. A dose of arsenic or anthrax bacilli does not invariably result in death. It is true that so much arsenic or so many anthrax bacilli usually cause death; but whatever quantity you name there is always someone who will either succumb to less or survive more. The most that can be said is that such and such a quantity is the minimum fatal dose for such and such a percentage of your population. Cause has ceased to be invariable and has become statistical.

With its invariability it has also lost its inevitability. If *A* does not always cause *B*, why should it ever cause *B*? Scientists have tried to bridge the gap by looking for inevitability among the entities of nuclear physics; but here too, frequency and not necessity is the most they can find. Logicians have pointed out that you can at least define *A* in such a way as to include all the cases in which it has been followed by *B* and exclude all those in which it hasn't. But no such redefinition will make it inevitable that your redefined *A* will be followed by *B* in the future—not even once.

It is for these reasons that the notion of cause is more and more rarely used in the sciences and is being replaced by notions such as correlation. But even on the supposition that the sciences could dispense entirely with causes, it does not follow that techniques can do the same.

The differences between sciences and techniques are not fully appreciated yet, and there is a tendency to think of techniques as if they were merely crude forms of sciences, or perhaps subdivisions

<sup>1</sup> For instance, by E. B. Strauss, 'Reason and Unreason in Psychological Medicine', *Lancet*, 1952, July 5 and 12, 1 and 49.

of them. This can be extremely misleading. Sciences consist of observations and the laws that are formulated to link them together. Techniques are systems of rules for achieving various kinds of ends. The science of mechanics observes, and formulates laws to describe, the behaviour of matter under the action of forces, without considering whether this behaviour is in any way useful or desirable. The technique of engineering consists of rules for achieving a number of useful objects, such as the construction of machines, dwellings, or bridges; and these rules are based on more than one science—mechanics, chemistry, meteorology, and so forth. Two things help people to confuse sciences and techniques. Sciences make extensive use of techniques in their practical experiments, while on the other hand the rules of some techniques are very complex and ‘scientific’ in their formulation. The other thing is the fallacy that sciences are really means to serve practical ends—in other words, glorified techniques. It is true that the laws and observations of sciences can be and are used for practical ends, and that this is often the historical reason why they were made; but we must distinguish factual statements and general laws from the practical precepts that can be based on them; and these precepts are techniques. Anyone who is doubtful about the logical soundness of this distinction has only to notice how very rare it is for the rules of any technique to be based on the laws and observations of any one science.

Medicine is often called a science, but is really a technique,<sup>1</sup> or rather two groups of techniques, one preventive and the other remedial. These are two systems of rules for achieving two allied groups of ends, the prevention and the cure of human disorders. Medicine is not a science—not a body of observations and laws—nor is it even based on a science of its own, but on several sciences—chemistry, biology, physics, genetics, psychology,<sup>2</sup> and no doubt others—none of which are used by this technique alone.

<sup>1</sup> It is sometimes called an art. When we call something ‘an art’ we are trying to say that it is a technique in which the rules are very hard to formulate. They are never impossible to formulate; as time goes on, more and more arts are reduced to techniques whose rules are formulated in manuals. For some reason it is thought more unusual and meritorious to practise a technique by intuition than by industry in learning. This may be why it is popular to call medicine an art. Certainly some operations in all techniques are carried out intuitively, and when they succeed the credit goes to intuition. When they fail, the blame is put on ignorance or incompetence.

<sup>2</sup> Not to be confused with psychotherapy, which is a technique.



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It is, of course, possible for remedial and preventive techniques to do without the notion of cause. But if they do they remain completely empirical. It would theoretically be possible to discover by sheer trial and error that the incidence of malaria can be reduced by getting rid of stagnant water or covering its surface with oil. But the chances of this discovery are greatly increased if the 'cause' of malaria is known. In remedial medicine too the chance of success is obviously greater if the 'cause' of the disorder is known. We can not only prevent but also arrest and improve rickets if we know that the cause is a dietary deficiency. The knowledge is useful of course only if it suggests a successful means to the technician's end. Occasionally it does not. The knowledge of the causes of some infectious diseases (such as bubonic plague) is of use in preventive medicine, but has not so far led to any effective remedial technique. Conversely, some not unsuccessful remedies have been discovered without a clear idea of the cause of the disorder: examples are the treatment of muscular rheumatism by heat or of some psychic disorders by prefrontal leucotomy.

Nevertheless we find it impossible to conceive of a really successful method of prevention or treatment that has nothing to do with the cause of the disorder. In some way, we feel, it must avoid or correct the abnormal state of affairs that is responsible for the symptoms, even if we do not know what that state of affairs may be or how the method affects it. We feel sure that when we know more about this particular disorder we shall find that the treatment which was discovered empirically corrects or compensates for some abnormality in that state of affairs, and we shall without hesitation conclude that this abnormality is the cause of the disorder. This feeling is far from illogical. It is based, although we do not fully realise it, upon a definition of cause which the remedial technician is able to employ but which is forbidden to the scientist. Whatever difficulties the latter may have in defining or justifying the notion of cause, the remedial technician, knowingly or unknowingly, is basing his whole procedure on a definition that runs like this:

'The cause of a disorder is an abnormal state of affairs on the restoration of which to normal the disorder disappears.'

This definition would not of course serve other kinds of technique as it stands; it would need a slight modification, for example, to suit preventive medicine. Some kinds of technique make little or no use

of the notion of cause. But for remedial techniques that are to progress beyond the purely empirical stage the notion, as I hope I have shown, is a necessity. As such, it requires no elaborate justification, and is not hard to define.

If I seem to have taken up a good deal of valuable space over the notion of cause in medicine, it is not so much of my own choosing as because physician-philosophers have made such heavy weather of it.<sup>1</sup> I do not deny that there are plenty of ambiguities in the way in which it is used, as we shall come to see when we consider the nature of the psychic causes which are used to account for certain somatic symptoms. But the idea that there is something fundamentally wrong with the notion itself springs from the failure to distinguish techniques from sciences.

### 3 *The Nature of Psychic Causes*

Perhaps the most noticeable thing about the psychic causes to which somatic symptoms are attributed is that they are almost always emotions. Halliday gives seven differentiae which distinguish psychosomatic from ordinary physical disorders, and the first of these is the frequency with which emotion is a 'precipitating factor'.<sup>2</sup> The very title of Flanders Dunbar's review of the whole literature of psychosomatic medicine is *Emotions and Bodily Changes*; nor have I succeeded in finding in it any case in which some mental phenomenon other than emotion is offered as a cause of disorder.

There are several features that are present in emotions and absent (or present to a much smaller degree) in other mental phenomena, such as sensation, recollection, or volition. One of these is the close association between emotions and somatic occurrences of a kind that are not regarded as disorders. The tears of grief, the flush of anger, the quickened pulse of fear, are not looked upon as pathological symptoms. At the same time, the emotions that cause these are the very emotions that are most commonly blamed for psychosomatic disorders. Indeed the association between emotions and changes in the vascular, respiratory, and endocrine systems is so close and constant

<sup>1</sup> The very word is becoming rare in medical literature. It is usually replaced by 'aetiology' or, more impressively, 'aetiologically relevant factor', but in such a way that it makes perfect sense to substitute 'a cause' for these phrases. Truly we all retain a primitive instinct which makes us dive into the jungle when we think we are being followed too closely.

<sup>2</sup> Halliday, *op. cit.*, p. 47



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that one of the most plausible of the behaviourist theories is the attempt to describe the emotions not as *causing* but as *being* these bodily changes. I do not want to make too much of this at this stage, but it serves to emphasise my point, which is that of all psychic phenomena emotions are the ones that look the least psychic and the most somatic.

Secondly, emotions are much more unreliable in their occurrence than other psychic reactions, such as perception or recollection. If you hold up a pair of scissors in front of Mr Smith and say, 'What have I here?', it is extremely likely that he will react by saying 'A pair of scissors'. Or if, next day, you ask him to recall what you showed him on that occasion, it is again highly probable that he will react by saying 'A pair of scissors'. It is true that there are states such as drunkenness, unconsciousness, or sleep in which he will not give the normal reaction, but these are easy to define and recognise. In contrast it is not at all easy to predict his emotional response to some event such as being told that he is a profligate wretch. He may exhibit amusement, fear, or indignation, depending on whether his accuser is his boon companion, his employer, or his wife. These reactions, too, may vary in kind or intensity according as the accusation is put to him across the breakfast table, the office desk, or the bar of a public-house. Again, Mr Smith or Mr Jones may react very differently in the same circumstances to the same accusation, whereas in matters of perception or recollection their responses would tend to be alike. It is true of course that there is a range of stimuli, such as a blow in the face or the death of one of his children to which almost every man will display the same emotional reactions in a wide variety of contexts; but these stimuli are infrequent extremes. This 'unreliability', as I call it, is a feature of emotions of which we are very well aware in everyday life, but which we tend to forget when we are in the laboratory or clinic, hunting for causes to explain symptoms.

The next point to notice is that the emotions which are sometimes offered as the psychic causes of symptoms are sometimes events and sometimes states. A man's illness may follow and be attributed to mortification at the loss of his job. But if two men in the same economic circumstances lose their jobs and only one falls ill, his illness may be attributed to his psychological state. In the former case the cause was said to be an event, in the latter a state, although in both cases the event was the same. It all seems to depend on whether the event is unusual enough to attract the attention of the seeker after

causes, or so ordinary that he is forced to conclude that the cause lies in the state of the person to whom the event happens. In the former case he will point to what I call an 'occasional' cause, in the latter to a 'static' cause. This ambiguity in the use of 'cause' is fully recognised by those who use the label 'psychosomatic' reflectingly. Halliday, for example, says :

Illness is regarded, not as a fault in the parts but as a *reaction*, or mode of behaviour, or vital expression of a living unit in response to those forces which he encounters as he moves and grows in time. Cause is therefore *twofold* and is to be found in the nature of the individual and the nature of his environment at a particular point in time.<sup>1</sup>

It is reassuring to find the 'static' and the 'occasional' senses so clearly distinguished, although Halliday seems to have an odd idea that this double sense of the word is appropriate only when it is used of living organisms, for he calls this the 'biological idea of cause'. In fact, of course, we use both senses when we are talking about the vicissitudes of inanimate objects. If I tap a piece of glass with a hammer and it breaks, I say that the tap was the cause of the breakage ; but if I tap several pieces of glass with the same strength and find that this is the only piece that breaks, I say that the cause is the unusual molecular state of that piece, which makes it brittle. But the fact that Halliday's point is true, though he did not realise it, of inanimate as well as animate matter, merely seems to me to confirm that it is a sound one.

Unfortunately it sometimes happens that static causes are confused with something else, namely, dispositions. In the case of the piece of glass that broke under the hammer, it is one kind of explanation to say that it did so because of its molecular state, and another kind to say that it did so because it was brittle. The former is an explanation in terms of a static cause, the latter an explanation in terms of a disposition. The brittleness which we attribute to the glass is merely a name for its disposition to do the sort of thing which it did—that is, to shatter under a light blow. This is a very different thing from its molecular structure. Anyone who thinks this distinction unreal has only to reflect that if he is asked, 'But why is it brittle?' it makes sense to reply, 'Because of its molecular structure'. Another way of describing the difference is to say that a static cause has effects but a disposition has only instances.

Nevertheless, in discussions of psychosomatic disorders psychic

<sup>1</sup> Halliday, op. cit., p. 367



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dispositions are often mentioned as if they were static causes. A patient with tachycardia is sometimes told that the explanation of his disorder is that he is an 'anxious type'. Now it makes sense to tell the patient that the cause of his tachycardia was an attack of anxiety which occurred just before or during it, for the attack of anxiety was an actual occurrence. And it makes a sort of sense to say that the attack of anxiety was an effect of his anxious disposition, although, as I have pointed out, it would be better to say that it was an instance of it. But it makes no sort of sense to say that an attack of tachycardia is an effect of an anxious disposition. If this means that it is an instance of this disposition, then we must be using the phrase 'anxious disposition' in the unusual sense of 'disposition to feel anxious or to have bouts of tachycardia or both', and if we are using the phrase in this sense we are not really offering a causal explanation of his tachycardia, but only offering something of which it is an instance.

Of course it may be that when we explain the attacks of tachycardia in this way we are really saying 'He probably has an attack of anxiety before or during them, and this is the cause'. As I have said, this makes sense. Or again we may mean 'He is of an anxious disposition because of his physical constitution, which is also the static cause of his attacks of tachycardia'. This probably makes sense too. Each of these possible interpretations fits best, as we shall see, into a particular view of the mind-body relationship.

Another striking feature of psychic causes is that it is sometimes possible to point to them and sometimes not. Sometimes the patient who complains of tachycardia admits that it follows or accompanies a bout of anxiety, but just as often he denies this. In this sort of case we are offered two kinds of explanation. We may be told that the patient is 'an anxious type'. As I have just pointed out, this is not a causal explanation unless it means 'He probably did feel anxious just then' (which in this case we know he did not) or else 'His tendency to have bouts of anxiety (although he didn't have one then) is the effect of a physical constitution of which another effect is this tachycardia'; and this would mean that his anxious disposition was not the cause of the tachycardia but merely, like it, another symptom.

### 4 *Unconscious Emotions*

The other explanation which we may be offered is that the patient did feel anxiety just before or during his bout of tachycardia, but that he was unaware of it—in other words, that it was 'unconscious'.

Sometimes this 'unconscious anxiety' is supposed to be a reaction to a contemporary stimulus and consequently an occurrence of limited duration. Sometimes it is spoken of as if it were a more or less permanent state dating back to some long-past trauma and act of repression.

The notion of unconscious emotions is a whole subject in itself, and I can do no more in this context than refer, by way of a reminder, to the main issues. For example, it is not generally realised that Freud himself had great difficulties over this notion. At first he used it unreflectingly; at least one of his earlier papers refers without hesitation to 'locked-up affect'. Later he came to the conclusion that affects could only be conscious or preconscious, partly for theoretical reasons which are of no interest for our present purpose, and partly because he seems to have reasoned that an 'unfelt feeling' was a contradiction in terms. The doctrine of 'no unconscious affects' embarrassed even some of his own followers (it was attacked, for example, by de Saussure in 1922) but has survived as part of orthodox psycho-analytic theory.<sup>1</sup> Nor has the notion of unconscious emotions escaped criticism from the philosophers. Russell<sup>2</sup> and Broad<sup>3</sup> have pointed out that many cases of what are called unconscious emotions are really either

- (i) conscious but misdescribed by their owner (jealousy, for example, thought of as 'healthy rivalry');
- (ii) conscious but ignored (as with people who do not admit, even to themselves, that they lost their temper a moment ago);
- (iii) not actual occurrences of an emotion, but a disposition towards one which is exemplified only in very exceptional circumstances, and not on most of the occasions when one would expect it (as Tennyson's young widow, for example, broke down only when her infant was placed upon her knee, and not when she heard of her husband's death).

Let us accept, however, for the sake of argument, the possibility of unconscious emotions, and consider another point. Are the emotions that are regarded as the causes of somatic symptoms conscious or unconscious? The answer seems to be that they can be either. Tachycardia is sometimes attributed to conscious, sometimes to unconscious anxiety. As a result believers in psychosomatic medicine

<sup>1</sup> Marjorie Brierly, *Trends in Psycho-Analysis*, London, 1951, p. 44

<sup>2</sup> Bertrand Russell, *Analysis of Mind*, 1921

<sup>3</sup> C. D. Broad, *The Mind and its Place in Nature*, London, 1925



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appear at first sight like a man who tosses a penny and calls 'Heads', but when the penny falls tails up says 'Of course I meant heads down'. The concept of unconscious emotion looks to the uncharitable as if it were designed for the very purpose of allowing someone who cannot produce a demonstrable psychic cause to say, 'Well it's there, but you can't see it'. Nor have those who use the concept in psychosomatic medicine given us as clear an idea as we should like of the relationship between conscious and unconscious emotions in their rôle as psychic causes of somatic symptoms. They do not tell us whether symptoms caused by unconscious emotions differ, either in type or intensity, from those caused by conscious emotions. Is psychogenic tachycardia more intense or prolonged when accompanied or unaccompanied by conscious anxiety? The dearth of literature on this interesting and important question is surprising.

Finally, is the concept of unconscious emotion essential to psychosomatic theory? It is not easy to answer this question. Some writers such as Halliday are able to express their views without using the word 'unconscious'; certainly I have not yet succeeded in finding it in his *Psychosocial Medicine*. These writers, however, make a good deal of use of the notion of 'psychological types' which are prone, they say, to particular kinds of somatic symptoms. This means that they are using a disposition to a certain kind of emotion as a psychic cause, and as I have pointed out this can make sense in only two ways. Either it means that because the man has a disposition towards an emotion he must have experienced it on a certain occasion, and so this conscious emotion, not the disposition, is the real psychic cause; or else it means that both symptoms and disposition are the effects of a constitution that reacts to certain situations in either or both of these ways, in which case of course the disposition is not the real cause. Now, the first of these interpretations offers genuine psychic causes. The trouble is that it may not fit the observed facts. It may be true that the man is prone to anxiety, but it may not be true that he felt anxious just before or during his bouts of tachycardia. The second interpretation, on the other hand, may fit the observed facts better, but it provides us with a psychic cause only if we think of this mysterious constitution as a mental and not a physical one—that is, as a state of some unobservable, unintrospectible mind-stuff. At this stage we are getting very close once more to the notion of unconscious emotions; and the interesting thing is that when we look closely at some descriptions of 'psychological types' we often find that

unconscious emotions have been smuggled back into them although, of course, under a disguise. Halliday, for instance, tries to avoid using the concept ; but here is an extract from his account of the way in which a man comes to have a constitution that gives rise to bronchial spasm :

. . . if the expiratory cry, the infant's call for comforting or reassurance, is frustrated by a rejecting mother figure, then the early need for a spiritual mother love is not lived through and the individual *retains within its being an unexpressed crying* which may be represented physiologically by an undue tendency to bronchial spasm.<sup>1</sup>

This is clearly an unconscious emotion described in other words. Notice too that the words are highly metaphorical ; it would be interesting to see how Halliday would translate them into literal English.

The situation seems to me to be this. If you want to offer a genuine psychic cause for somatic symptoms, and at the same time to account for cases in which this cause is not observable, you probably cannot do without the concept of unconscious emotions. Nor is it really sufficient to make use of the concept : you must believe that it is a picture of something that exists in reality. For it is possible to argue that unconscious emotions—and other unconscious mental entities—do not correspond to anything in reality, but are merely 'scientific models', like isobars or contour lines, which are justified solely by their usefulness in the explanation, prediction, and improvement of human behaviour.<sup>2</sup> But if you hold this view you are not really offering a psychic cause, but are saying 'I am going to talk as if there were such things as psychic causes, but you will understand that they are merely useful fictions'.

I have dealt very briefly and tentatively with what seems to me to be the three big issues that arise over the notion of unconscious emotions in psychosomatic medicine—its logical legitimacy, its relationship to the nature or intensity of the symptoms, and its indispensability to the theory of the subject. Obviously there is room not only for much more discussion of the first and last of these questions but also for practical research on the second. The rôle of this concept in psychosomatic medicine is really a subject within a subject, and all that I hope to do is to make these issues sufficiently clear to prevent them from acting as red herrings during the rest of this discussion.

<sup>1</sup> Halliday, *op. cit.*, p. 93 (my italics)

<sup>2</sup> I myself have put forward arguments in another place for taking this view of the Freudian unconscious.



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### 5 *Dualism versus Monism*

We can now turn from the notion of psychic causes to the other source of controversy over the standard definition of psychosomatic disorder—the divergent views on the relationship between body and mind. This is a vast and fertile field, but fortunately we do not have to dissect all the varieties of theory that have been cultivated by the metaphysicians. We can ignore idealism, psycho-physical parallelism, neutral monism, and epiphenomenalism, and concentrate on the two main theories on which popular and therefore medical metaphysics are based.

We must begin with Cartesian dualism, if only because of its seniority. I shall not try to disguise my distrust of it, and as a summary of it I shall quote Ryle's masterly analysis :

When Galileo showed that his methods of scientific discovery were competent to provide a mechanical theory which should cover every occupant of space, Descartes found in himself two conflicting motives. As a man of scientific genius he could not but endorse the claims of mechanics, yet as a religious and moral man he could not accept, as Hobbes accepted, the discouraging rider to those claims, namely that human nature differs only in degree of complexity from clockwork. The mental could not just be a variety of the mechanical.

He and subsequent philosophers naturally but erroneously availed themselves of the following escape-route. Since mental-conduct words are not to be construed as signifying the occurrence of mechanical processes, they must be construed as signifying the occurrence of non-mechanical processes ; since mechanical laws explain movements in space as the effects of other movements in space, other laws must explain some of the non-spatial workings of minds as the effects of other non-spatial workings of minds. The difference between the human behaviours which we describe as intelligent and those which we describe as unintelligent must be a difference in their causation ; so, while some movements of human tongues and limbs are the effects of mechanical causes, others must be the effects of non-mechanical causes, i.e., some issue from movements of particles of matter, others from workings of the mind.<sup>1</sup>

Cartesian dualism is by no means obsolete (the theory outlined by Kapp<sup>2</sup> is one which Descartes himself might have expounded if he too had had the benefit of a modern scientific education) ; and as we

<sup>1</sup> Gilbert Ryle, *The Concept of Mind*, London, 1949, pp. 18-19

<sup>2</sup> R. O. Kapp, *Mind, Life and Body*, London, 1951

shall see later its influence on medicine is still profound, probably because of the mistaken belief that it is the only metaphysical position consistent with Christianity.

Be that as it may, the Cartesian picture of the mind as an invisible pilot in the cockpit of a complex machine became a little less fashionable during the nineteenth century, when the spurt of scientific discovery began to make people wonder how much work the pilot really did. They took to pieces cruder organic machines with simpler cockpits, or even watched them at work, and began to see how it might be possible to explain the working of these machines without the necessity of bringing in a pilot. Nor did their religious beliefs insist that these cruder machines must carry a pilot. Finally the human machine, when taken apart, proved to consist of the same kinds of parts, although of more elaborate design, as the cruder machines. People began to ask whether there really was a pilot in the human machine, and to suggest that all its behaviour could be explained without this assumption. This was the age of material monism.

Early in this century, however, it began to be realised that there were certain disorders that could be cured or alleviated by talk. These disorders were of a rather special kind, and the talk that was found to alleviate them was also of a special kind. The patient had to lie on a couch and talk about the childhood incidents and emotions that he found it most difficult and repugnant to recall. The controversy about the efficacy of this technique and about the exact conclusions that can be drawn from it is still going on. One by-product, however, of the notoriety of the new technique was the re-instatement of the concept of mind in popular and medical metaphysics. The reasoning must have been this. Apparently you could affect the functioning of the human body by contriving that it should voice certain ideas and emotions. Ideas and emotions were beyond a doubt things of the mind; you could not examine them by microscope, electro-encephalograph, or any other scientific instrument, but only by introspection. Therefore there must be something more than a machine and a cockpit, for apparently you could talk to the pilot and through him influence the functioning of the machine.

People had always known of course that you could influence a human body by talking to it. You could persuade it, for example, to pluck and eat the fruit of a certain tree by uttering certain words in its presence. The disorders, however, which the Freudian technique seemed to alleviate were ones that had proved intractable to the



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techniques of talking *to* the human body. Some of them were disorders of behaviour which ought at first sight to have been corrected by reasoning with their owner, but were not. For example, the man who refused to go out of doors for fear of open spaces should have been cured by being told that he was in no danger in open spaces : but he was not cured. The other kind of disorder was one which nobody expected to be cured by talking to the person ; these had been regarded as disorders of the body and not of behaviour. The classic instances were hysterical paralyses or anaesthesiae. Finally, there was one more striking thing about the Freudian technique. It seemed to consist more of talking *by* than of talking *to* the human body. All these things, I think, combined to account for the way in which popular metaphysics were induced by the Freudian discoveries to abandon material monism and look for another bolt-hole. It was this effect upon popular metaphysics which earned Freudianism the suspicion of the orthodox scientist, although at the same time the churches regarded it as a blow for and not against materialism.

The best bolt-hole was the one probably invented by Spencer (with the help at a later date of Lloyd Morgan), who saw life and mind as new qualities which 'emerged' at different stages in the process of evolution as Darwin had described it. The 'emergence theory' regards mind and body neither as two completely different kinds of thing nor as one and the same thing. Their relationship was rather that of two extreme manifestations of one sort of thing. The popular analogy for this rather ill-defined state of affairs was that of a scale. A well-known physician says

. . . for practical purposes the distinction between mind and body at each end of the scale must be maintained in the same way that it is easier to think of energy or matter, structure or function, space or time, heredity or environment, or in terms of organic or inorganic nature. . . .<sup>1</sup>

The point was that there was no place at which anyone could say 'Now we are crossing over from mind into body'. It was like a spectrum of all the shades of green that result from mixing the two colours, blue and yellow. At no point can you say 'This is all blue' or 'This is all green'. The most you can say is 'This is mainly blue' or 'This bit has more blue in it than that bit'.

The emergence theory is thus an attempt to compromise between

<sup>1</sup> A. E. Clark-Kennedy, *Medicine*, London, 1947, i, p. 36

dualism and monism. It says in effect 'Mind and body appear to be different things because you usually look at one end of the scale or the other. But if you look closer you will find that there is no sharp division between them; they merge gradually into one another. Therefore they are really one.' Stated in this way, the theory is obviously dualism trying to escape its own disadvantages. It does not make mind and body one any more than shading blue into yellow by lighter and lighter shades of green makes them all one colour.

In the last decade a new view has developed which now underlies so much of the medical thinking in this country and North America that it might almost be called the official view of Anglo-American medicine. I call this view 'functional materialism'. It identifies the distinction between mind and body with the distinction between 'structure' and 'function'. This is, at first sight anyway, a simple distinction. A structural description of a clock is one that can be given without seeing it work and without knowing how it is intended to work. It describes the size, shape, materials, and relative positions of the wheels, screws, springs, and other parts. A functional description, on the other hand, tells you what the clock does; it mentions the diurnal movements of the hands and the striking of the hours and quarters. In the same way it is possible to give both a structural and a functional description of a living organism. A purely structural account can be given by the anatomist as a result of his dissections on the slab; his picture is, as it were, a 'still' photograph. The functional description can be given only by someone who has seen the organism working; it is a 'moving' picture. 'Functional materialism' says that some of the ways in which the human and other organisms function are what we call 'mind'. Obviously not all functions, even of human beings, are what we should call 'mind'. The normal circulation of the blood, normal respiration, normal digestion, and so on are not 'mind'. Other functions equally certainly are 'mind'; talking (aloud or to oneself), playing chess, and so on; all the things which the Cartesian would call 'mind-directed'. Note this difference between Cartesianism and functional materialism; instead of calling some functions 'mind-directed', the functional materialist says 'They are mind'.

This view has recently been given the precise logical exposition it needed in Ryle's *Concept of Mind*. In this important book, which attempts the final overthrow of Cartesianism by a logical revolution, Ryle argues that in distinguishing mind from body as if they were



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two kinds of substance, Descartes was making what he calls a 'category mistake', the same sort of mistake as that of the foreigner watching his first game of cricket who is told the functions of the bowlers, the batsmen, the fielders, the umpires, and the scorers, but then asks to be shown the man whose job it is to exercise the team spirit. As Ryle says,

Certainly exhibiting team spirit is not the same thing as bowling or catching, but nor is it a third thing such that we can say that the bowler first bowls and *then* exhibits team spirit or that a fielder is at a given moment *either* catching *or* displaying *esprit de corps*.<sup>1</sup>

Mind, according to Ryle, is not another kind of substance, and anyone who thinks it is is making this sort of category-mistake. Like Romeo's surname 'It is not hand nor foot Nor arm nor face nor any other part Belonging to a man'. It is a name, and apparently a misleading name, for some of the more complex of the ways in which our bodies behave.

In this brief history I have tried to show how the fashionable view of the mind-body relationship has swung like a pendulum from Cartesian dualism to material monism, then back again (but not all the way) to the modified dualism of the emergence theory, and now back again (but once more not all the way) to a modified materialism, which I have nicknamed 'functional'. From the logician's point of view these four stages consist of

- (i) an attempt to make the distinction between mind and body one of distinction in kind of substance ;
- (ii) an attempt to show that there is no second substance to be distinguished ;
- (iii) an attempt to draw a distinction of degree ;
- (iv) the conclusion that it is really a distinction in kind after all, but in kind of concept, not kind of substance.

If this seems an oversimplified account of the way in which the pendulum has swung between dualist and monist views of the mind-body relationship, remember that it is concerned not with the metaphysics of metaphysicians but with the metaphysics of the medical profession, which are much closer to popular metaphysics.

In the remainder of this paper I shall consider how the standard definition of psychosomatic disorder fits into popular Cartesianism and popular Ryleanism, and whether there are any preferable alternative definitions.

<sup>1</sup> Ryle, *op. cit.*, p. 17

6 *Cartesianism and the Definition of Psychosomatic Disorder*

The definition of psychosomatic disorder as 'somatic symptoms with psychic causes' is obviously one that comes most easily from the lips of a Cartesian dualist. If you believe in two kinds of substance so different that they cannot be confused with one another, and if you know that emotions are made of one kind and somatic symptoms of the other, you have a neat answer to most queries. Can you always be sure of distinguishing cause from symptom? Yes, for one belongs to one kind of substance and one to another, and never the twain shall be confused. Can you be sure that the cause is psychic? Yes; emotions are things of the mind, and the very close association between them and certain somatic occurrences, such as tears or sweat, is interesting but not important. Unconscious emotions too are much easier for a dualist to swallow than for a material monist. A monist who uses the notion must always be wondering whether he is really talking about physical structure and whether the notion is not simply a diagrammatic way of representing this structure. But a dualist need have no such qualms. He can believe in a mental constitution which is responsible for dispositions to feel conscious emotions just as he believes in a physical constitution which is responsible for his shortness of breath when he climbs a hill. He can even believe that behind the stage where conscious mental events are enacted there is a mental 'back stage' where other, less presentable, scenes take place; in other words he can even believe in unconscious mental events.

He will have some difficulty, it is true, in explaining how it is that some symptoms—somatic and psychic—can be treated with a measure of success by either physical or psychotherapeutic methods. He may answer that in such cases one or other method is merely treating the symptoms while the other goes to the cause, and this may well be so in a great many instances. But there are disorders such as asthma in which psychotherapy is often effective and yet in which it is difficult to maintain that all physical treatment is merely symptomatic; and this defence becomes even more far-fetched in the case of psychic aberrations which can be treated by drugs or electroconvulsion as well as by psychotherapy.

Broadly speaking, however, the difficulties of the conception of psychosomatic medicine for the Cartesian are the difficulties of Cartesianism itself. These are well-known. Chief among them is the difficulty of explaining how two so different substances can interact at



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all. Does this interaction take place at one point in the middle of the brain, as Descartes himself supposed? Or are there many points of interaction, as latter-day Cartesians like Kapp seem to believe? Does interaction take place without diminishing or increasing the total amount of energy in the organism? These and other difficulties have been discussed time and again—for example, by Broad or, less impartially, by Ryle. All that I am concerned to point out is that the standard definition of psychosomatic disorder which we are considering is born of dualism and it is therefore not surprising that, with the possible exception I have just mentioned, it gives its parent no trouble. Whether it is likely to survive its parent's demise is another question, which I now want to discuss.

### 7 *Functional Materialism and the Standard Definition*

How does the standard definition fit into functional materialism? At first sight there appears to be a place ready-made for it. The medical profession is already accustomed to distinguish between 'functional' and 'organic' disorders. A work that has probably done more than any other single one to mould the ideas of the modern doctor says

Functional diseases are disorders in which some organ or system does not act properly, although after death no change can be found to account for the condition. Thus functional paralysis may come on in a limb though the nerves, brain, muscles, etc., may apparently be quite healthy. The heart may beat feebly and with irregularity, although the valves are sound and the muscle of the heart shows no disease, this also being a functional disorder. It is in the case of nervous disorders that so-called functional disorders most abound; thus mental disorders, such as hysteria, are purely functional, and epilepsy may be so. The contrast to 'functional' disorder is 'organic' disease, in which some definite change, such as a tumour, or diseased heart-valve, or degeneration of heart-tissue, etc., is found to account for the symptoms of the case. The treatment of functional disease is more satisfactory now that medical psychology is becoming established on the sound foundation of observed fact.<sup>1</sup>

There are of course difficulties in the distinction itself. Clark-Kennedy<sup>2</sup> argues that 'structure' is merely an extreme form of

<sup>1</sup> 'Functional Diseases', art. in Black's *Medical Dictionary*, ed. J. D. Comrie, London, 1941

<sup>2</sup> Clark-Kennedy, loc. cit., pp. 19-20, 338

'function'. 'Structure', he says 'is only a relatively static form of function on which function of a more active kind is often superimposed'. He points out that what we regard as the structure of such organs as our bones is really in 'a continuous process of calcification and decalcification' which is, he maintains, merely a slower sort of function than the processes we usually think of as functional.

This attempt to reduce the distinction to one of degree is not very impressive. The fact that the present structure of a bone is merely one stage in a lifelong process of calcification and decalcification does not make it at all difficult to distinguish this structure, which can be studied on the operating-table or the pathologist's slab, from the creation of new cells and the loss of old ones. It may be that Clark-Kennedy is simply recalling what he has read of Heraclitus. But whatever view you hold on this point, you must, I think, give up any neat and intellectually satisfying idea of translating the standard definition of psychosomatic disorder into 'structural symptoms with functional causes'. There are, it is true, some disorders among those labelled 'psychosomatic' in which the symptoms could be called 'structural'—for example, the gastric ulcer itself, or a psychogenic skin eruption. But we could not stretch the term 'structural' to cover such symptoms as tachycardia or asthma. Nor, for that matter, could we call all functional causes 'psychic'.

But since the functional materialist believes that mind is a name for some of the ways in which our bodies function, we feel that he ought to be able to define the particular kind of malfunctioning which is responsible for the symptoms of psychosomatic disorders, and thus to translate 'psychic cause' into his own language. Three such translations have been attempted. The commonest is one which, in effect, says that 'psychic cause' equals 'malfunctioning of the nervous (or, sometimes, central nervous) system'. This view cannot be lightly dismissed. Even Neolithic man knew that there are plenty of somatic symptoms that are connected with the presence of visible abnormalities, such as wounds or tumours, in the central nervous system, which can be relieved by surgical treatment of that organ. From observations such as these it is an easy step to the assertion that all somatic symptoms that can be shown not to be attributable to the malfunctioning of another organ are attributable to the malfunctioning of this one.

Can this kind of malfunctioning be demonstrated in the same way as the malfunctioning of other organs, such as the heart, can be de-



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monstrated by instruments like the stethoscope or electrocardiograph? Some people would answer 'Yes; it can be demonstrated by the electro-encephalograph'. But is this really so? The results of this sort of investigation have been conveniently summarised by Margaret Kennard,<sup>1</sup> and, although electro-encephalography is a highly specialised and technical subject, one or two interesting points emerge very clearly. Abnormal EEGs (electro-encephalographic records) are found in a substantial percentage (between 5 per cent and 15 per cent) of adults 'with no known focal organic pathology of the central nervous system and no overt psychopathology'. The percentage of abnormal EEGs is higher among adults with known organic disorders of the nervous system, and also among psychotics, psychopaths, psychoneurotics, and patients with peptic ulcers, sclerodema, or allergic disorders. This seems to favour a theory of cerebral malfunctioning in psychosomatic disorders. But if the evidence is examined a little more closely it is not so satisfactory. First, the percentage of abnormal EEGs among normal adults is quite substantial. Secondly, the percentage among the others is nowhere near 100 per cent. It is probably highest, as might be expected, among those with known organic disorders of the central nervous system, but in many experimental groups was well below 50 per cent among the psychotics and neurotics (as low as 6 per cent in Kibbe's series). Thirdly, some doubt has recently been cast on the value of EEGs recorded while the subject is supposed to be in a relaxed, unstimulated state. This has been criticised on two grounds: on one hand, that the EEGs should have been recorded during stimulation likely to arouse the behaviour characteristic of the subject; and on the other, that in the experiments with relaxed subjects the abnormal EEGs were merely due to the undetected failure of some of the subjects to relax. Certainly this is one plausible explanation of the widely differing results of several experiments. Fourthly, one of the features which was regarded by these experimenters as an abnormality was a high proportion of what are called 'alpha-rhythms'; but it has recently been suggested that this phenomenon has an essential and normal function in sensory perception, analogous to that of scansion in a television set<sup>2</sup> (Kennard does not mention this). Finally, an examination of the EEGs of 136 patients who were undergoing successful psycho-analytic treatment

<sup>1</sup> Margaret Kennard, 'The Electroencephalogram in Psychological Disorder', *Psychosomatic Medicine*, 1953, 15, 95

<sup>2</sup> See W. Grey Walter *The Living Brain*, London, 1953

showed 'no appreciable change . . . in type of EEG for a given individual . . . over a period of years'.

The case, therefore, for attributing psychosomatic disorders to cerebral malfunctioning is not yet nearly strong enough to allow the functional materialist to define them confidently as 'somatic symptoms due to malfunctioning of the central nervous system'. It is quite conceivable that the case will grow weaker with further investigation. If, for example, successful psychotherapeutic treatment of patients with psychosomatic disorders and abnormal EEGs were found to leave the EEGs just as abnormal (as happened with the 136 patients who were being psycho-analysed for psychic disorders) this would suggest that EEGs had nothing to do with the disorder or at most indicated a type of person likely to succumb to it, which is very different from what we are looking for. Nor would it surprise me to find the case getting weaker. I have already pointed out that psychosomatic disorders are always associated with emotion of one kind or another, and that among psychic events emotions are the least psychic and the most somatic. It may also be true that of all cerebrally-controlled events they are the least cerebral.

But suppose for the sake of argument that the evidence for cerebral malfunctioning in psychosomatic disorders were really convincing. Would this be the end of the functional materialist's difficulties? Probably not. You will remember that he is supposed to be providing a 'technical cause'—that is, a cause which is described in such a way as to assist the technician in working out or improving upon his remedial technique. If we assume for the sake of argument that cerebral malfunctioning is the cause of psychosomatic disorders, and if we also assume for the moment that the only possible remedial techniques are physical, the notion of cerebral malfunctioning will give the technician all the help that he wants. He could then think about the likely locations of the malfunctioning and the advantages of using surgery or drugs to isolate the affected part or restore its normal functioning. But the notion is of no use to the technician who is using psychotherapeutic methods and who therefore has to think in terms of emotions, desires, memories, and phantasies (whether they are conscious or unconscious and whether he calls them by these names or uses the technical terms of psycho-analysis). He cannot translate these into alpha-rhythms or harmonic responses or vice versa. If, of course, psychotherapeutic methods were known to be so inferior to physiological treatment of the central nervous system that they were



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definitely a 'second best', the uselessness to them of the notion of cerebral malfunctioning would not matter so much. But at present this is far from being the case; physiological treatment of the central nervous system is employed only in the more severe cases of disorder, which are usually psychic rather than somatic in their outstanding symptoms.

It may be argued, of course, that this definition is merely an example of theory being ahead of technique, and that there will come a day when physiological techniques will be able to make the delicate corrections of the abnormalities in the brain that are responsible for such symptoms as peptic ulcer. Quite apart, however, from the likelihood of such technical developments, this argument assumes that if cerebral malfunctioning is the psychic cause it is associated with structural abnormality; for instance, that somewhere or other in the brain, be it in cortex or hypothalamus or scattered throughout its subdivisions, there are a few hundred thousand synapses whose thresholds require to be raised or lowered. But is it certain that the abnormality is of this structural kind at all? The recent development of cybernetics and information theory has shown that there are several ways of designing machines that will imitate in some degree the behaviour of organisms with a central nervous system, and have thus provided new suggestions for the ways in which our brains may work. Hitherto we have tended to assume that difference of function must always be associated with difference of structure; for example that if one office adding-machine functions correctly and another of the same make functions incorrectly there is some structural difference between them. This is true of machines of this kind, but may not, apparently, be true of electronic calculating devices. If I understand correctly such writers as Wiener<sup>1</sup> or Sluckin<sup>2</sup> it seems to be possible for two structurally identical machines of this type to function differently because of the differences in the 'information' that is 'reverberating' in their delay line storage circuits, and even for some kind of oscillation to cause faulty functioning of a number of structurally correct circuits. I do not want to make too much of this point; I merely use it to emphasise that until we know more about the way in which the brain *does* function (as distinct from the ways in which it *might* do so) we cannot rule out the possibility that *some* kinds of cerebral malfunctioning may not be associated with structural

<sup>1</sup> Norbert Wiener, *Cybernetics*, London, 1948

<sup>2</sup> W. Sluckin, *Minds and Machines*, London, 1954

abnormality at all. This would mean, if it turned out to be so, that it was not only practically impossible, as at present, but also theoretically impossible to describe exactly what was wrong with the individual in terms of anything but the 'reverberating information' itself (that is, in terms of the individual's memories, phantasies, etc.); and this in turn would probably mean that it was not only practically but also theoretically impossible to treat him otherwise than by altering the 'information'—in other words, by communicating semantically with him, as in psychotherapy.

We cannot safely assume, therefore, that it will ever be possible to describe 'psychic causes' of disorders in terms of structural cerebral abnormality. What we can safely assume is that it will be a very long time indeed before we know enough to dispense in all types of case with descriptions of psychic malfunctioning in psychotherapeutic terms.

We must now consider two other ways in which functional materialists sometimes try to translate 'psychic cause'; they will not detain us long. These are both attempts to avoid the difficulty of equating 'psychic cause' with 'malfunctioning of a particular organ'. The first attempt begins by explaining that in the functioning of any and all organs and parts of the body there are some events that we can describe by simple laws like those of dynamics—for example, the falling of the raised arm when its muscles are relaxed. Others require the laws of chemistry to describe them, as in the case of metabolism. In the same way, runs the argument, there are functions that can be described only by the laws of psychology; examples are talking, writing, dancing. In this way psychic functioning is not identified with the functioning of one particular organ, but is regarded as an aspect of the functioning of all parts of the body, including of course the central nervous system, just as another aspect is the metabolic functioning of all these parts, again including the central nervous system.

This way of talking has its attractions and advantages. It takes neatly into account the way in which emotions, even in normal subjects, involve other organs of the body in addition to the nervous system. But when it is faced with the problem of psychic malfunctioning does it really offer an alternative to the way of talking which identifies this with malfunctioning of the central nervous system? The crucial point is whether it regards malfunctioning as traceable to a particular organ. If it does, then where does the abnormality lie?



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Where is tachycardia bred, in the heart or in the head? If in the head, we are back amongst the difficulties we have just left. If in the heart, how are psychotherapeutic methods ever successful?

But some functional materialists seem to me to hold a different theory. Instead of attributing psychosomatic disorders to malfunctioning of the central nervous system or of any other organ or organs of the body, they believe, I think, that the interaction of two or more properly functioning organs can result in symptoms which we regard as disorders, in much the same way as the working of a wireless set can be impaired if a battery of the wrong voltage is installed; there is nothing wrong with any of the parts, taken by themselves, and it is simply the combination that is wrong. My own name for this theory is 'malcombination'. You will not find it stated so crudely by those who hold it, but it often seems to me to underlie the view that is sometimes expressed about this or that psychosomatic disorder—namely, that one cannot do more than treat the symptoms. It is essentially a defeatist theory, since it names as cause something that cannot very well be altered. Fortunately there is a certain amount of evidence against it; for it is hard to see how, if this theory were true, psychotherapeutic treatment could ever be effective.

It seems to me, therefore, that when the functional materialist is faced with the task of translating the standard definition of psychosomatic disorder into his own language, he is in a serious dilemma. If he translates 'psychic cause' as cerebral (or neural) malfunctioning, he is automatically limiting the concept to the sort of cause that can be tackled by physiological but not by psychotherapeutic techniques. If, on the other hand, he translates it as 'psychological malfunctioning', and by this means something other than organic malfunctioning, he is limiting the concept to the sort of cause that can be tackled by psychotherapy but not by physiological methods. He may even be relapsing, with or without realising it, into dualism.

### 8 *A Definition to Fit Functional Materialism*

In the face of this dilemma some functional materialists have abandoned the attempt to distinguish psychic causes within the organism and have taken refuge in what I call the 'total reaction' definition. This runs on something like these lines—

'A psychosomatic disorder is one in which somatic symptoms are part of the individual's total reaction to a psychological stimulus.'

Instead of pointing to a psychic cause within the organism this points to a psychological cause outside it. It has a close connection with the 'malcombination theory' because it is the sort of definition in which malcombinationists may take refuge. But whenever you come across someone who talks about a psychosomatic disorder as a disorder 'of the total organism' and not of mind or body considered in isolation, and so forth, it is highly likely that he subscribes to this sort of definition.<sup>1</sup> It is also possible to be attracted to it simply because it does not commit you to a precise view as to the nature, location, or treatment of these disorders.

Consider first the concept of a psychological cause. Events outside the organism can quite conveniently be subdivided according to the way in which they affect it. There are events such as being knocked down by a bus or blown over by a gust of wind which affect the person in a very simple way and can be adequately described in the laws of dynamics; call these dynamic causes. In the same way other events, such as contact with a corrosive acid, can be called chemical causes, and others again biological causes; for example, contact with a parasite or virus. At the top of the scale come the events whose effects upon the person cannot be adequately described in terms of the laws of dynamics, chemistry, biology, and so forth, but require a group of laws which we must call psychological. If a man who is walking along a road stops when he sees a notice called 'Danger', the effect of the notice cannot be explained in terms of dynamics, chemistry, or biology. It can be partly explained in terms of optics, but to explain it fully it is necessary to bring in terms like 'learning', 'meaning', and so on. It is of course necessary, when talking of causes in this group, to realise that the word 'psychological' is being used in a very wide sense. You may argue, for instance, that the effect of seeing a notice 'Trespassers Will Be Prosecuted' can be understood with the aid of a little knowledge of English law, and without psychology. This is so; all I want to point out is that the definition we are considering uses the word 'psychological' not in the narrow sense in which it is reserved for behaviour that cannot be explained by any respectable profession, but in the wider sense in which it applies to all behaviour that can be explained only by using such words as 'meaning', 'learning', and so on. Those who object to this use can employ some such word as 'semantic' instead.

<sup>1</sup> See, for example, Andras Angyal, *Foundations for a Science of Personality*, pp. 61-62.



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Consider next the concept of total reaction. The most obvious question which this raises is 'How are we to know what should and what should not be included in the total reaction to a given psychological stimulus?' If the psychological stimulus is the loss of my job, what is my total reaction to it? Suppose that I experience at the time no more than relief at getting out of a boring and frustrating routine, and perhaps determination to find something better, but in a few days I develop digestive troubles. Are the latter to be regarded as part of the total reaction, although they did not occur at once? Suppose as well that I was in a Japanese prison camp during the war, and that my digestion, which had till then been healthy, has since troubled me in this way. Should my gastric disorders be regarded as part of my total reaction to the prison camp or to the sack? These questions certainly demand answers, but I do not think they are very hard to give. The rule is, I suppose, that you must regard a symptom as part of the total reaction to a stimulus if you think that without that stimulus (or a very similar one) the symptom would not have manifested itself when it did. On this criterion, of course, my gastric disorder could be regarded both as part of my total reaction to the prison camp and also as part of my total reaction to the sack: but I do not see why the same symptoms cannot be part of the total reactions to two different stimuli, even if one is psychological and the other of some other kind.

From the point of view, therefore, of the philosopher who has forsaken Descartes for Ryle, or of the scientist who by embracing functional materialism has in effect done the same, the total reaction definition is probably not unsatisfactory, and is certainly more satisfactory than the standard one. But there is another point of view to be considered, that of the technician who sets himself the task of preventing, curing, or at least alleviating the sort of disorder we are trying to define. As I pointed out when I touched on the notion of cause in medicine, the technician must, if he is not to be completely empirical, have the cause of the disorder as his starting-point for working out a *rationale* of treatment or prevention. We have seen that Cartesian dualism met this need by providing a psychic cause, a notion that with all its difficulties and ambiguities at least gave the technician something he could attack. Does the total reaction definition do the same? The answer is that it does not abolish the notion of the cause of the disorder, but relegates it from within the organism to its environment. This is quite clearly of little help to the technician.

It is true that if his task is preventive it helps him to know that his patient's symptoms are part of his reaction to getting the sack ; he can at least advise him to avoid this in future. But circumstances are seldom so simple that this is effective ; the patient may well react in the same way to other threats to his security, and some of these may be impossible to avoid. It is also true that there are still some fields of medicine in which the only advice the technician can give is preventive. So far as I know, there is only one cure for bubonic plague, and that is not to get it. Fortunately there are reliable ways of following this advice. But this does not mean that technicians in such fields have confined their attention to the environment that is part of the cause of such disorders. On the contrary, they are all the more intent upon finding some factor within the organism that can be altered in such a way as to confer immunity to such disorders or make recovery more likely.

I have already emphasised Halliday's point that 'cause is two-fold and is to be found in the nature of the individual and in the nature of his environment at a particular point in time'. The total reaction definition is unsatisfactory to the technician because it says nothing about the nature of the individual which explains why his reaction to a particular stimulus should include abnormal somatic symptoms. We have seen the logical difficulties that led to this reticence, but we must also recognise that this reticence deprives the therapeutic technician of a definite idea of the sort of target within the organism at which he should aim his technique.

### 9 *A Technician's Definition*

The remedial technician, therefore, who is offered the total reaction definition is faced with four courses. He can resign himself to a policy of ameliorating the symptoms themselves ; this is regarded in medicine as a last resort, and almost the worst adjective that can be applied to a method of treatment is 'symptomatic'. His second course is sheer empiricism, of which the disadvantages are obvious. His third choice is to reject functional materialism and to return to dualism, probably with the feeling that he is doing so for the wrong reason. His fourth, and most enterprising, course is to find a working definition, a technician's one, that will not be inconsistent with functional materialism but at the same time will recognise the possibility of treating psychosomatic disorders by both physiological and psychotherapeutic methods.



## THE DEFINITION OF PSYCHOSOMATIC DISORDER

I said at the outset that present-day medicine has two ways of classifying disorders—by symptom and by cause. This is the assumption on which the textbooks are written, as if they were teaching a science instead of a technique based on several sciences. But in practice the technicians themselves, without realising it, employ a third method of classification. Two patients may both exhibit symptoms which are diagnosed as those of gastric ulcer ; yet the same consultant may decide that one patient should be treated surgically and the other medically. The former is thenceforth labelled by doctors and nurses as a 'surgical case', and the other as a 'medical case', and they may even be allotted to 'surgical' and 'medical' wards. This is classification by method of treatment. It is used in all techniques that are worth the name, although the process is often so simple and instinctive that it may seem pretentious to call it a method of classification. But the golfer who diagnoses his next task as 'a brassie shot' is doing exactly the same as the consultant we have just been considering.

As soon as we recognise the possibility of this method of classification, a definition of psychosomatic disorder suggests itself :

'Psychosomatic disorders are somatic symptoms which can be successfully treated by methods effective in treating psychic symptoms.'

There are one or two things to note about this definition. First, it presupposes that we have no difficulty in distinguishing between somatic and psychic symptoms. This is surely so. If a man claims to see a dagger before him, the handle towards his hand, when we see no such thing ; or if he murders without adequate provocation, we do not hesitate to call these psychic symptoms, in contrast to the flutterings of tachycardia or the pantings of asthma. The difficulties only arise when we have to distinguish somatic from psychic causes, and that we are not required by this definition to do.

Secondly, the phrase 'methods effective in treating psychic symptoms' clearly includes not only psychotherapeutic techniques but also physiological ones, such as prefrontal leucotomy, insulin therapy, and electro-convulsion. The objection may be made that these two groups of techniques are quite different in kind, one dealing with the patient's mind, the other with his body. It will be obvious, however, on second thoughts that this is a dualistic objection, whereas we are now concerned with the problems raised by the abandonment of dualism. The fact that the definition includes such diverse methods of treatment seems to me to be in its favour.

What does the definition mean by 'can be successfully treated'? Success in treating disorders of this kind is notoriously difficult to assess. Sometimes it is simply a matter of individual preference. Patient *A* and patient *B* may both suffer from dyspepsia, but *A* may go to a psychotherapist who makes conscious his anxieties, while *B* goes to a physician who puts him on a diet. The result in both cases may be freedom from dyspepsia, but *A* may now be worrying with his head instead of his stomach and *B* may be deprived of all his favourite dishes. Some readers might prefer to be *A*, some *B*. Sometimes again the preference for one or other type of treatment is understandably determined by the severity of the symptoms. A man who suffers from mild or occasional headaches usually contents himself with a proprietary preparation of acetylsalicylic acid and phenacetin, but if they are violent and incessant he may be driven to a psychotherapist. The ideal of all remedial technicians must of course be the complete and permanent abolition of all symptoms without impairment of normal function. But very often both the physician and the psychotherapist must be content with reducing the frequency or the severity of the symptoms without placing so many restrictions on the patient's behaviour that he cannot go on working or enjoying himself.

With all these reservations, however, it is still possible to point to a good many cases in which psychotherapy brings a greater measure of success than physical therapy, just as there are cases in which psychotherapy would be ridiculous. There will always, however, be an intermediate range of cases in which the comparative value of physical and psychotherapeutic methods will be disputed, and I want to say something about these.

Too often the contestants in this sort of dispute think that the result of it (if it is conclusive at all) will determine

- (i) that psychotherapy is the only method of value, and that physical methods are useless, or *vice versa*; and
- (ii) that this applies to all cases of the disorder in question.

I do not think that (i) needs much consideration before it is seen to be an assumption which is both unnecessary and untrue. Unnecessary, because only a dualist *can* hold that there are disorders which are inaccessible to physical interference, and not even a dualist *must* hold this. Untrue, because it is extremely rare to find a disorder in which the symptoms respond to nothing but psychotherapeutic treatment, even though the results of physical treatment, such as drugs, may be temporary or in some other way unsatisfactory.

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The assumption that whatever conclusion you come to in this sort of dispute must apply to all cases of the disorder in question is equally but not so obviously false. There is no logical reason for assuming that because one case of asthma responds well to psychotherapy all others will respond better to this technique than to any other, or that because one case of gastric ulcer is cured by surgery psychotherapy is ineffective in treating this disorder. For one thing, the value of psychotherapy in dealing both with psychic and with somatic symptoms varies with the age, intelligence, and other circumstances of the patient, just as the value of surgery also varies, although it depends on rather different factors : a surgeon frequently decides not to operate on a patient whose advanced age or constitutional weakness makes his response to the operation uncertain.

Some may hesitate, however, to accept the doctrine that the best technique for treating the same disorder may vary from patient to patient, and may take refuge in the argument that in such cases we are mistaken in thinking that we are dealing with the same disorder. Attempts are made to distinguish asthmas that respond to desensitisation from asthmas that respond to psychotherapy, and to give them different labels. Similar distinctions are drawn in the case of peptic ulcers, colitis, skin conditions, and so on ; some of these distinctions are more convincing than others. The difficulty about this argument is that the symptoms of the cases labelled psychogenic are often as similar to the symptoms of the other cases as these are to each other ; the only ground for distinguishing them seems to be the very fact that this argument is trying to explain, namely that they respond best to different kinds of treatment. If the distinction is intended to mean no more than this, then I have of course no quarrel with it, for it is the basis of my technician's definition. But if the distinction is meant to imply that the disorders are somehow different in nature, and that therefore there is one brand for which psychotherapy is the only cure and another brand for which it can never be a cure, I should demand independent evidence of this.

It is true that the best method of treatment will vary not only with the patient but also with the technician. Just as surgery nowadays is a very highly skilled technique which, except in emergencies, is practised only by those with special training and experience, so psychotherapy also requires special qualifications. Certainly every general practitioner can and does employ psychological methods, wittingly or unwittingly, with every patient, if only because he must communicate



with him ; and there is no doubt that some general practitioners would make good psychotherapists in the specialised sense. But there are doctors who could never hope to deal psychotherapeutically with psychic or somatic symptoms, just as there are others who could never perform any but the simplest operation. Now if Dr *X* were the only doctor in the world, then the question whether Mr *Y*'s asthma is psychosomatic would depend on how good a psychotherapist Dr *X* was ; and if Dr *X* were a bad psychotherapist there would be very few psychosomatic disorders. As it is, the medical profession is both numerous and willing to pool its resources ; the physician and surgeon will frequently pass on a patient to the psychotherapist, either before or after trying their own techniques on him, and the psychotherapist occasionally returns the compliment.

When these apparent difficulties have been disposed of, the case for the technician's definition of psychosomatic disorder can be put very simply. What is the most convincing demonstration that a disorder is psychosomatic ? Clearly it is the successful treatment of that disorder by psychotherapy ; that is, by the methods evolved to deal with psychic symptoms. This being so, there can be no sounder basis for a definition.

I am not suggesting that a similar method of classification should be used to distinguish various kinds of non-psychosomatic disorder, where the present custom of classifying by symptom and cause seems to serve fairly well. It is only because so many of the disagreements over psychosomatic disorders arise from the logical consequences of trying to define them in the customary way, as if mind were something that could be put in a pathologist's jar like a diseased appendix, that I have pointed out another way of defining them. And although this definition was designed to meet the needs of functional materialism, it is one which can be employed by a dualist—or indeed, so far as I can see, by the holders of almost any metaphysical view of the mind-body relationship. This may commend it to those who have personal experience of the way in which metaphysics can bedevil the exchange of useful information.

### *Summary*

Because we classify other disorders by symptoms and by causes, and tend to divide people into minds and bodies, we think of psychosomatic disorders as somatic symptoms with psychic causes. Apart

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from some ambiguities in the notion of psychic causes, chiefly due to the fact that in the case of this kind of disorder they are always regarded as emotions, this definition fits quite well into a dualist view of the mind-body relationship, although this has its own difficulties.

It does not fit so well into the modern monistic view, 'functional materialism'. From the scientific point of view it would be possible to redefine this kind of disorder as 'somatic symptoms that are part of a person's total reaction to a psychological stimulus'. But medicine is a technique and not a science, and this definition gives the technician no indication of a cause within the person which he can attack.

A better definition from his point of view would be 'somatic symptoms that can be successfully treated by methods effective in treating psychic symptoms'. This helps to remove disagreements arising from the way in which the same symptoms in different patients respond best to physical treatment in one case and psychotherapy in another, and is at the same time consistent with both dualist and 'functional materialist' views on the mind-body relationship.

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# THE LOGIC OF QUANTA\*

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## *Introduction*

THERE are many roads that lead to quantum theory,<sup>1</sup> and the easiest avenue of approach has yet to be determined. The motive for the introduction of the quantum hypothesis was the paradox of *energy equipartition* of classical mechanics according to which the average thermal energy of particles has a common value,  $3kT$ , at temperature  $T$ , no matter whether the particles are loosely or tightly bound. This would imply, however, an abrupt decrease, from  $3kT$  to zero, when a very tightly bound particle becomes entirely fixed. Experience shows that the thermal energy of a particle is a *continuous* function of the tightness of its bond, that is, of its vibration frequency  $\nu$ . This continuity of the average thermal energy as a function of  $\nu$  can be ascribed, according to Max Planck, to quantised individual energy values,  $E = 0$  or  $E = h\nu$  or multiples thereof. The search for a justification of this strange quantum hypothesis gave rise to a thorough re-examination of classical statistical mechanics during the first decade of this century in the hope of escaping from the paradox of energy discontinuity. When all efforts in this direction failed, physicists became resigned to Planck's oscillator rule and later also to Niels Bohr's orbital quantum rules as fundamental—as irreducible to anything more simple and plausible.

The same attitude prevails today toward the modern quantum prescriptions of Born, Heisenberg, and Schrödinger, namely, (i) the general matrix formalism which is equivalent to the superposition principle of wave mechanics, and (ii) the Born commutation rule,  $qp - pq = h/2i\pi$ , which is equivalent to the Schrödinger operator rule,  $p = (h/2i\pi)d/dq$ . The Schrödinger wave equation is a combination of (i) and (ii). But just because it is a close-knit combination of heterogeneous elements it has puzzled many of those trying to analyse what the physicists are doing. We therefore prefer to follow Dirac<sup>2</sup> in making a sharp distinction between (a) the general metric of the

\* Received 18.iii.55

<sup>1</sup> Sir Edmund Whittaker, *History of Aether and Electricity: Modern Theories*, Edinburgh, 1954

<sup>2</sup> P. A. M. Dirac, *Principles of Quantum Mechanics*, Oxford, 1935



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probabilities of transition, and (b) the specific rules of quantum dynamics dominated by periodicity rules in which the constant  $h$  occurs. The present article will stress the following new points of view :

- (a) The necessity for abandoning strict determinism and for introducing laws of *probability* can be traced back to a fundamental law of *entropy-continuity* ; this requires the admission of a continuity of equality values between the various states of a mechanical system, so that two states may either be equal, or totally unequal, or of a 'fractional equality'. A certain degree of fractional equality between two states signifies a corresponding degree of 'fractional separability' of the two states, and furthermore a fractional probability of transition from one to the other state under a 'test'. From the (real) probabilities it is only a small step to the (complex) probability amplitudes  $\psi$  connected by a superposition law. The latter is not a new independent physical law, but is mathematically implicit in the metric of the (real) probabilities.
- (b) The dynamical qualities of mechanical systems depend on the relation between 'conjugate' observables, namely, between co-ordinates  $q$  and momenta  $p$ . Instead of the deterministic equations of motion of classical mechanics, one has merely to postulate that  $q$  and  $p$  yield a constant probability density in  $qp$ -space. This postulate, familiar from classical statistical mechanics, combined with the general probability metric of section (1) leads by mathematical necessity to the *periodic* relationship between co-ordinates and momenta, i.e. to the most characteristic feature of quantum dynamics.

The strange periodicity rules of Planck, de Broglie, Schrödinger, and Born, which were originally introduced as *ad hoc* hypotheses, can thus be reduced to simple and immediately acceptable fundamental principles. Of course, what is strange and what is plausible and natural may be a matter of dispute. There are those who consider the modern quantum rules as the normal state of affairs and deny that there is any need for further scrutiny. Inquisitive students who ask for a justification are told that the quantum rules 'work' in practice ; or they are referred to three articles of faith : *duality*, *uncertainty*, and *complementarity*. The present article is to show that quantum theory can be understood as a consequence of two principles, namely (a) *entropy-continuity*, and (b) *mechanical conjugacy*.

I *The Probability Metric*(a) *Continuity Principle and Transition Probabilities*

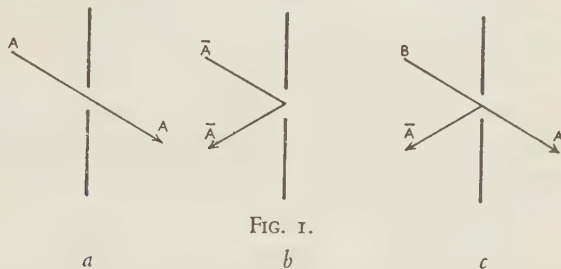
It is not surprising that a theory dealing with statistically controlled phenomena should be deducible from an amendment to the entropy rather than from an amendment to the energy law of thermodynamics. The principle of entropy-continuity maintains that the following discontinuity of classical theory is unrealistic: classical theory maintains that the entropy of diffusion of two gases  $A$  and  $B$  is either zero when the gases are equal ( $A = B$ ), and has a constant value,  $2R \log 2$  per mole of either gas, when  $A$  differs from  $B$  ( $A \neq B$ ), irrespective of the magnitude of the difference between  $A$  and  $B$ . If this statement were true, it would imply an abrupt drop of the entropy, from  $2R \log 2$  to zero, as one goes from the case of two very slightly different gases to that of two equal gases. In order to avoid this entropy-discontinuity, known as the Gibbs paradox, one must admit a continuous scale of various degrees of 'fractional equality' ( $A \sim B$ ) signified by diffusion entropies intermediate between  $2R \log 2$  and zero. The chain of reasoning which leads from here to a general theory of transition between states controlled by statistical rules has been described at other places<sup>1</sup> in detail, and will be reiterated now only in brief outline.

Whether two states  $A$  and  $B$  of the same kind of particle qualify as equal or different depends on whether they are inseparable or separable by some selective device which we shall call a 'filter'. Let us define an ' $A$ -filter' as a device which passes particles in the state  $A$  and rejects them when they are in a state non- $A$ , written,  $\bar{A}$  as illustrated by the schematic Figs. 1a and 1b. The passing and rejecting of particles is meant in a general sense as any *yes* or *no* response of the device. In order to avoid the discontinuity from a situation where *all* particles pass when they are in a state  $A$  to the case where *none* pass when they are in a state ever so slightly different from  $A$ , it is necessary to admit states  $B$  intermediate between  $A$  and non- $A$ , such as that of Fig. 1c, where a certain fraction of the incident particles is transmitted and the rest is rejected.

<sup>1</sup> A. Landé, 'Continuity, a Key to Quantum Mechanics', *Philosophy of Science*, 1953, 20, 101: 'Quantum Indeterminacy, a Consequence of Cause-Effect Continuity', *Dialectica*, 1954, 8, 101. See also the author's Monograph, *Foundations of Quantum Theory: a Study in Continuity and Symmetry*, Yale University Press, 1955.

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The statistical *passing fraction*  $P(B, A)$  of  $B$ -particles through an  $A$ -filter defines the *fractional equality* between the states  $A$  and  $B$ . Since an equality is mutual, one will expect  $P(B, A) = P(A, B)$ , that is, the statistical fraction of  $B$ -particles passing an  $A$ -filter is as large as the fraction of  $A$ -particles passing a  $B$ -filter. Such *symmetry* of  $P$  is required for reasons of thermal equilibrium.



Furthermore, it is plausible to expect that those  $B$ -particles which in passing the  $A$ -filter behave *as though* they were in the state  $A$  actually *are* in the state  $A$ , or rather have jumped from  $B$  to  $A$  in contact with the  $A$ -filter, thereby becoming qualified for passage. Similarly, the rejected  $B$ -particles must have jumped from  $B$  to  $\bar{A}$  = non- $A$  in order thereby to qualify for rejection. This is indicated in Fig. 1c which represents the true intermediate between Figs. 1a and 1b. The *passing fraction*  $P(B, A)$ , identified before with the *equality fraction* between  $A$  and  $B$  thus is the statistical fraction of  $B$ -particles jumping to  $A$ , under an  $A$ -filter test (an ' $A$ -test'). For an individual  $B$ -particle the same  $P(B, A)$  is the *probability of transition* from  $B$  to  $A$ , under an  $A$ -test.

Any two states  $A$  and  $B$  of a particle (or mechanical system) can either be equal,  $P(A, B) = 1$ , or quite unequal,  $P(A, B) = 0$ , or fractionally equal,  $0 < P(A, B) < 1$ . If this is so, then one can select from the totality of *all* states of the particle a set of mutually quite unequal states  $A_1, A_2, A_3$  (i.e. of states whose mutual equalities are zero) :

$$\begin{aligned} P(A_k, A_{k'}) &= 0 \quad \text{for } k \neq k', \\ &= 1 \quad \text{for } k = k' \end{aligned} \tag{I}$$

From the remaining states one can select another set of states  $B_1, B_2, B_3 \dots$  which are also mutually quite unequal, separable, or to use the technical expression, *mutually orthogonal*. Then one can pick out other orthogonal sets  $C$ , and  $D$ , and so forth, until each state of the



particle is given its place in one of the orthogonal sets. States belonging to different sets, such as  $A_k$  and  $B_j$  are fractionally alike :

$$P(A_k, B_j) = P(B_j, A_k) < 1 \quad (2)$$

One may draw up a table or *matrix* (3) listing the mutual equality fractions  $P(A_k, B_j)$  between the states of the two sets  $A$  and  $B$  :

$$\begin{pmatrix} P(A_1, B_1) & P(A_1, B_2) & \dots & P(A_1, B_M) \\ P(A_2, B_1) & P(A_2, B_2) & \dots & P(A_2, B_M) \\ \dots & \dots & \dots & \dots \end{pmatrix} = \{P(A, B)\} \quad (3)$$

It is a matter of course that the sum of the transition probabilities from any one  $A_k$  to  $B_1$  and  $B_2$  and  $B_3$ , etc., is *unity* ; the same holds for the sum of the probabilities from any one  $B_j$  to all the states  $A_1$  and  $A_2$  and  $A_3$ , etc. :

$$\sum_j P(A_k, B_j) = 1 \quad \text{and} \quad \sum_k P(A_k, B_j) = 1 \quad (4)$$

We have arrived here at the formal sub-structure of the quantum theory. It is a structure usually assumed *ad hoc* and justified by experience. We have seen, however, that it follows by simple reasoning from the postulate of *entropy continuity* which maintains that the paradoxical entropy discontinuity of Gibbs does not occur in the real world.

### (b) *Physical Details*

The formal scheme developed so far deals with the various 'states' in which a given object, or mechanical system or particle, can be found when it is tested by a 'filter' or another specially constructed device referred to as a measuring instrument or *meter*. An  $A$ -meter is an apparatus built so as to yield a continuity of possible responses  $A, A', A'', \dots$  along an  $A$ -scale. When the  $A$ -meter responds to the tested object at the mark  $A_k$ , then the object is said to be in the state  $A_k$  as a result of this test. According to quantum terminology, an object thus found in a state *remains* in this state when left alone. If a second  $A$ -test is now applied, the object will certainly be found still in the same state  $A_k$ , since  $P(A_k, A_k) = 1$ . If instead the object is submitted to a  $B$ -test, it may be found either in  $B_1$  or  $B_2$  with probabilities  $P(A_k, B_j)$ .

All this holds only for 'well-defined states' ; unreasonable implications of the statement 'no change of state occurs unless the system is subjected to test' are due to an unjustified application of these principles to states which are not well defined. For example, a state of position  $xyz$  of a mass point in a given field is not a well

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defined state, but a state of position at a given time is. There is a definite probability relation  $P(A_k, B_j)$  between the state  $x_k y_k z_k t_k$  and the state  $x_j y_j z_j t_j$ , where it is assumed that these two states are determined by different meters : a position meter at time  $t_k$  is considered as different from a position meter at time  $t_j$ . Only well defined states are connected by definite probabilities of transition ; *vice versa*, when two states are connected by definite transition probabilities then they are well defined. We shall omit the adjective 'well defined' from here on, as we have omitted it in Section 1a. To mention other examples : states of energy of a mass point in a given field are not well defined states, but states ascertained by a combination of an energy meter, an angular momentum meter, and a meter for the  $z$ -component of angular momentum are (well defined) states, denoted by the spectroscopist as states ( $n l m$ ).

In principle, there is no difference between an object and a testing meter. In practice, however, the meter ought to be large and massive in comparison with the tested object, so that the reaction of the test on the meter is negligible. In short, the  $A$ -meter ought to be macroscopic (almost classical as it were), so as to permit responses  $A$  along a continuous range of  $A$ -values. A given small object and its copies, when tested with such an  $A$ -meter, *may* display only a selected set of states  $A_1, A_2, A_3 \dots$  characteristic of the object and denoted as the proper or eigen-values of the quality  $A$  of this object. Fields in which the object dwells are considered as belonging to the object. *Every test wipes out all memory of previous states.* Again we speak only of well defined states.

Our discussion is of a general character ; it deals with unspecified objects, unspecified states, meters, probabilities, etc. Also, the words 'when' and 'then' and 'now' do not necessarily denote a temporal sequence. This is implicit in the symmetry of the probabilities  $P$  with respect to the 'initial' and 'final' state of a transition. Of course, the experimentalist can carry out only one test *after* another. Hence he is primarily interested in states observed at various times, e.g. positional states in space-time, and on the other hand, in data which are conservative in time, such as energy and angular momentum.

### (c) Irreversibility and the Direction of Time

Classical mechanics with its equations of motion is symmetrical with respect to past and future. All efforts to define a preference of the future over the past at least in a statistical sense are doomed to

failure. One cannot turn symmetry of individual development into dissymmetry for the millions.

To be more specific, consider a large number of specimens of a gas in various states. If this *ensemble* comes out of the hands of physicists who may have deliberately chosen gas samples which at time  $t_0$  have low entropy values, then the overwhelming majority of the gases will indeed show, at a later time  $t_1$ , an entropy  $S_1 > S_0$ . But here the past is deliberately left out of consideration, and it does not prove anything about a dissymmetry.

Suppose, however, the samples of gas are left alone for a long time-interval. If inspection without reaction were possible, the entropies in each sample would be seen to fluctuate up and down, and the entropy curves would not show a marked difference between what may tentatively be called the  $+t$  and the  $-t$  direction.

To be still more specific, suppose the inspectors agree to call  $t_y$  an instant when the great majority of the gases happen to display a low entropy value  $S_y$ . The instant  $t_y$  may lie *between* two other instants  $t_x$  and  $t_z$ . But it is left open which of them is before and which is after  $t_y$ . Will the statistical record of the average gas entropies  $S_x$ ,  $S_y$ , and  $S_z$  give a clue to the time order? It will not: the overwhelming number of gases will show  $S_x > S_y < S_z$ , symmetrical in  $t_x$  and  $t_z$ . A much smaller number of gases will show  $S_x < S_y < S_z$ , but an equal number will show  $S_x > S_y > S_z$ —again no decision. With a very rare number of gases observers will find  $S_x < S_y > S_z$ —again symmetrical with respect to  $t_x$  and  $t_z$ . This consideration, first given by P. and T. Ehrenfest, shows convincingly that the individual as well as the statistical record will be *symmetrical* with respect to past and future.

A direction of time *is* defined within the framework of quantum theory.<sup>1</sup> A gas and its entropy do not change at all as long as the gas is not observed (objections to this statement can usually be refuted by asking 'what do you mean by an unobserved intermediate state?'). But when tested with an *A*-meter, such a test will produce a new situation, leaving  $n_1$  particles in the state  $A_1$  and  $n_2$  particles in the state  $A_2$ , and so forth. The occupation numbers  $n_1$ ,  $n_2$ , . . . determine the entropy  $S_A$ . Suppose now that three tests are carried out in a definite time order  $t_x < t_y < t_z$  with a great number of gas samples. The record of the corresponding entropies then will show in the over-

<sup>1</sup> From the large amount of literature on this subject we quote only A. Grünbaum 'Time and Entropy', *Am. Scientist*, 1955, **43**, 550.



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whelming number of cases  $S_x < S_y < S_z$ . This result is a mathematical consequence of the rules (4) for the transition probabilities; for these ensure that any initial distribution  $n_1, n_2, \dots$  over the states  $A_1, A_2, \dots$ , produced by an  $A$ -test, will be converted by a subsequent  $B$ -test to a new distribution of the particles over the states  $B_1, B_2, \dots$  with occupation numbers  $m_1, m_2, \dots$ ; and in such a way that the entropy  $S_B$  is larger than  $S_A$ . When  $t_B > t_A$ , then  $S_B > S_A$  in the overwhelming number of cases. Likewise the  $S$ -record of any large *ensemble* of gases tested at times  $t_x, t_y$ , and  $t_z$  showing  $S_x < S_y < S_z$  corresponds in the overwhelming number of cases to the time order  $t_x < t_y < t_z$ . The classical  $S$ -curve, supposedly based on inspection without infringement, is unrealistic. Actually, every inspection produces a re-distribution of the particles resulting in a higher entropy.

### (d) *Probability Amplitudes and Superposition*

The probability amplitudes  $\psi$  have acquired a reputation for being almost super-natural messages from an esoteric world of abstract irreality, guiding the particles of the real world to their proper places by invisible strings stronger than the laws of ordinary mechanics. The following discussion may help to dispel this myth. Let us first consider a geometrical analogy.

Points  $A, B, C, \dots$  in Euclidean space are connected by mutual distances  $P_{AB}, P_{AC}, P_{BC}$ , etc. When two distances  $P_{AB}$  and  $P_{BC}$  are given, they do not determine the distance  $P_{AC}$ ; yet neither is the latter entirely free, since there is the triangular restriction:

$$P_{AO} \leq P_{AB} + P_{BC} \quad (5)$$

This inequality represents a general law applicable to any three points; generality means *symmetry* with respect to all points, that is, one can replace the letters  $ABC$  by any other letters referring to other points without invalidating (5).

The triangular inequality (5) is always valid. There is the following elegant way of replacing this inequality between the definite quantities  $P$  by an equality between indefinite quantities. Associated with every quantity  $P_{AB}$  is another quantity  $\psi_{AB} = -\psi_{BA}$  called a *vector*. When  $P$  is given, the direction of the corresponding vector is variable, undetermined, many-valued. Only the absolute value of  $\psi$  is defined as  $|\psi| = P$ . The variable directions of the three vectors

$\psi_{AB}$ ,  $\psi_{AC}$ , and  $\psi_{BC}$  for three given distances can always be *adjusted* so that the following equation holds :

$$\psi_{AC} = \psi_{AB} + \psi_{BC}. \quad (6)$$

This equation, like the inequality (5), is a general relation : it holds for all triangles ; it is *symmetric* in all points. An equation is always more pleasant to look at than an inequality because of the feeling of security it conveys to the reader. But (6) simply hides the fact that the quantities  $\psi$  are themselves undetermined. Both (5) and (6) are *equivalent* expressions of the same distance-metric between points. The vector addition (6) cannot be considered as a new and unexpected discovery in geometry. If the vector addition rule 'works' for triangles both big and small, regular and irregular, *one should not be surprised* ; it works because the directions of the vectors are *adjusted* so that it works ; and they always *can* be so adjusted because the  $P$ 's satisfy the inequality (5).

In the special case that all points are confined to one *plane* one may introduce a complex notation,  $\psi = Pe^{i\phi}$  where  $\phi$  indicates the directional angle. This notation does not make  $\psi$  any more esoteric than the usual representation by an arrow. Still, a novice may be startled when told that geometrical relations can be represented by

$$\psi_{AC}e^{i\phi_{AC}} = \psi_{AB}e^{i\phi_{AB}} + \psi_{BC}e^{i\phi_{BC}}, \quad (7)$$

until he finds out that this is the same as (6) in the case of *plane* geometry.

The addition rule (6) is of such symmetry that one can replace the 'intermediate point'  $B$  on the right by any other intermediate point  $D$  or by several intermediate points, without affecting the result on the left :

$$\psi_{AC} = \psi_{AB} + \psi_{BC} = \psi_{AD} + \psi_{DC} = \psi_{AB} + \psi_{BD} + \psi_{DC} = \text{etc.} \quad (8)$$

The intermediate points in this general vector addition rule are *irrelevant* or 'virtual'.

We are now sufficiently prepared to discuss an interrelation between the probability tables  $\{P_{AB}\}$ ,  $\{P_{BC}\}$ , and  $\{P_{AC}\}$  of (3). Although  $\{P_{AC}\}$  may not be uniquely determined by  $\{P_{AB}\}$  and  $\{P_{BC}\}$ , there are mutual relations between the three  $P$ 's. For example, when the rules (4) apply to  $\{P_{AB}\}$  and  $\{P_{BC}\}$ , then these rules must also hold for  $\{P_{AC}\}$ . This is a rigid restriction to the desired interrelation law between the  $P$ -matrices, just as the inequality (5) imposed a restriction on the relation between the sides  $P$  of a triangle. By analogy with the geometrical example, one can now proceed in the following fashion.

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Associate with every quantity  $P(A_k, B_j)$  a vector  $\psi(A_k, B_j)$  of magnitude  $|\psi| = \sqrt{P}$  and of a variable direction. All vectors  $\psi$  shall be in *one plane*. Hence one can use the complex symbol

$$\psi = \sqrt{P}e^{i\phi}.$$

Two vectors  $\psi_{kj}$  and  $\psi_{jk}$  (introducing an abbreviated notation) shall have directions  $\phi$  and  $-\phi$  respectively.

Draw up tables

$$\left\{ \begin{array}{ccc} \psi(A_1, B_1) & \psi(A_1, B_2) & \dots \\ \psi(A_2, B_1) & \psi(A_2, B_2) & \dots \\ \dots & \dots & \dots \end{array} \right\} = \{\psi_{AB}\}$$

similar to the  $P$ -table (3), with the vector directions left undetermined. Then adjust the variable directions in the three tables  $\{\psi_{AB}\}$ ,  $\{\psi_{BC}\}$  and  $\{\psi_{AC}\}$ , so that the following *multiplication* theorem is satisfied (compare with the addition theorem (6)) :

$$\{\psi_{AC}\} = \{\psi_{AB}\} \times \{\psi_{BC}\}. \quad (9)$$

This formula is meant as a 'matrix multiplication', namely, in detail :

$$\psi(A_k, C_m) = \sum_j \psi(A_k, B_j) \times \psi(B_j, C_m) \quad (10)$$

with a sum of products on the right describing the transition from the state  $A_k$  to  $C_m$  *via* the complete orthogonal set of intermediate states  $B_1, B_2, \dots$  as indicated by the scheme :

$$A_k \rightarrow C_m = A_k \begin{array}{c} \nearrow B_1 \searrow \\ \rightarrow B_2 \rightarrow \\ \searrow B_3 \nearrow \end{array} C_m \quad (11)$$

The product of two vectors,  $\psi_1$  and  $\psi_2$  is defined in (10) as a vector of magnitude  $|\psi| = |\psi_1| \cdot |\psi_2|$  and of direction  $\phi = \phi_1 + \phi_2$  in the same plane. The multiplication rule (9), (10) is *general*, i.e. symmetrical in all sets of states. In particular, one may replace the intermediate set  $B$  by any other intermediate set  $D$ , or by several intermediate sets in succession, without affecting the result on the left of the equations :

$$\begin{aligned} \{\psi_{AC}\} &= \{\psi_{AB}\} \times \{\psi_{BC}\} = \{\psi_{AD}\} \times \{\psi_{DC}\} \\ &= \{\psi_{AB}\} \times \{\psi_{BD}\} \times \{\psi_{DC}\}, \text{ etc.} \end{aligned} \quad (12)$$

in perfect analogy to (8). The intermediate sets of states are *irrelevant*, they are 'virtual' in this connection.

The  $\psi$ -multiplication, in particular the form (10), is also known as the *superposition law for probability amplitudes*.



The multiplication or superposition rule (9), (10) for probability amplitudes is not a new empirical law. It is only another way of saying that there are sets of entities (= states of a system) connected by quantities  $P$  (= probabilities of transition) the sum of which is *unity* in accordance with (4); this includes the special cases  $P = 0$  characterising the members of any one set as mutually 'orthogonal', or totally different, separable, mutually exclusive. The fact that the relation between orthogonal sets of entities can be described by a law of orthogonal transformation (namely, the  $\psi$ -multiplication law) is not an empirical discovery any more than the vector-sum rule in a triangle. As long as the triangular inequality (5) for the distances  $P$  is satisfied one can *adjust* the directions of three vectors  $\psi$  so that they will conform to the addition rule (6); similarly, so long as (1) and (4) hold for the probabilities  $P$ , one can *adjust* the directions of corresponding probability amplitude vectors  $\psi$  so that the matrix multiplication (9), (10) holds.

This analogy between the vectors  $\psi$  of geometry and of quantum theory is of course known to the experts. But instead of communicating this knowledge to their students, they prefer to expound another rather lofty geometrical analogy, that of the abstract Hilbert space, which is of little help to the beginner unless he happens to be a mathematician.

### (e) *Virtual Intermediate States*

Suppose many copies of the same object are all in the state  $A_k$  and are now exposed to a  $B$ -meter; the latter will produce transitions to the states  $B_j$  with probabilities  $P_{kj}$ . After having been found in the states  $B_1, B_2, \dots$  respectively, the objects may now be tested with a  $C$ -meter yielding transitions from the state  $B$  to  $C_m$  with probability  $P_{jm}$ . The total probability of an object starting from  $A_k$  and arriving in the final state  $C_m$  is the sum of products  $\sum_j P_{kj} P_{jm}$ . If the intermediate meter is a  $D$ -meter displaying states  $D_1 D_2 \dots$  then the probability of arriving from  $A_k$  at  $C_m$  *via* the states  $D_n$  is  $\sum_n P_{kn} P_{nm}$ . It is not surprising that these two sums have different values, and that they differ from the direct transition probability from  $A_k$  to  $C_m$  as well as from that *via* both sets of intermediate states  $B$  and  $D$ :

$$P_{km} \neq \sum_j P_{kj} P_{jm} \neq \sum_n P_{kn} P_{nm} \neq \sum_j \sum_n P_{kj} P_{jn} P_{nm}. \quad (13)$$

In contrast, (12) maintains that the corresponding expressions for probability-amplitudes all yield a common value

$$\psi_{km} = \sum_j \psi_{kj} \psi_{jm} = \sum_n \psi_{kn} \psi_{nm} = \sum_j \sum_n \psi_{kj} \psi_{jn} \psi_{nm}. \quad (14)$$

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irrespective of the choice of the intermediate set or sets of orthogonal states, whether it is the set  $B$  or  $D$  or both or none at all. It would be erroneous to believe, however, that the transition from  $A_k$  to  $C_m$  actually takes place *via* the states  $B_j$  or *via* the states  $D_n$  (both cannot be true at the same time). And yet, this very mistake is made when it is said that the Schrödinger time-dependent differential equation is a 'process equation', which allegedly connects a *present* probability distribution with a *future* probability distribution. The Schrödinger equation does nothing of the kind. Indeed, how could there be a development in time aiming at a future probability distribution when the objects 'do not know' whether the final test will be a  $C$ -test giving them a choice of aiming at the states  $C_1, C_2, \dots$ , or whether the final test will be carried out with a  $D$ -meter offering final choices  $D_1, D_2, \dots$ . Those alleged intermediate states which are replaceable by other sets of states without influence on the result (14) are 'virtual'. By this is meant that they are helpful in the mathematical calculation of an unknown  $\psi_{km}$  when one supposes that the corresponding  $\psi_{kj}$  and  $\psi_{jm}$ , or  $\psi_{kn}$  and  $\psi_{nj}$  on the right of (14) are known—just as it is helpful for finding the vector  $\psi_{AC}$  when one knows the vectors  $\psi_{AB}$  and  $\psi_{BC}$ , or  $\psi_{AD}$  and  $\psi_{DC}$ , without actually going from  $A$  to  $C$  *via* the intermediate points  $B$  or  $D$ . Schrödinger's equation does not describe processes from an initial to a final state *via* intermediate states actually passed through. *There can be no process equation* when the final result depends on the final analysing meter. For this reason it is in the spirit of quantum theory to say that an object does not change its state after a first test until it is tested again.

### (f) *$\psi$ -Functions do not represent States*

A frequent source of misunderstanding is the inconsistent terminology of calling a  $\psi$ -function a 'state function'. One reads<sup>1</sup> that 'a state is, by definition, a  $\psi$ -function' and similar pronouncements. Yet from the very beginning of quantum theory the term *state* has been associated with a definite *datum* or combination of data ascertained by a meter or combination of meters. There are states of energy, states of position, or momentum, and so forth. In this established terminology,  $\psi$  is neither a state, nor a representation of a

<sup>1</sup> Gustav Bergmann, 'The Logic of Quanta', in H. Feigl and M. Brodbeck, *Readings in the Philosophy of Science*, New York, 1953. The present article tries to simplify Bergmann's presentation.

state.  $\psi$  is a statistical link between two states, e.g. between  $A_k$  and  $B_j$  which occur in symmetrical fashion in the probability

$$|\psi_{kj}|^2 = |\psi_{jk}|^2 = P_{kj}.$$

A function  $\psi_k(x)$  represents the statistical link between one state  $A_k$  and various states  $x_j$ , where  $|\psi_k(x)|^2$  is the probability of transition from  $A_k$  to the position  $x$  per unit  $x$ -range, and vice versa, from unit  $x$ -range at  $x$  to  $A_k$ . The notation  $\psi_k(x)$  conceals the symmetry of  $A_k$  and  $x$  in the probability relation. Further confusion is produced when one writes  $\psi(x)$ , where the reference to the one state  $A_k$  is omitted and emphasis is placed only on the other states  $x$ . Such a function  $\psi(x)$  is supposed to describe a 'pure state'; the latter is a mere academic construction. One may indeed write down on paper any function  $\psi(x)$ ; but in all applications one needs a definite reference to the omitted state which is linked with the states  $x$  by the function  $\psi(x)$ . No wonder that others have had a hard time trying to make sense of the inconsistent shorthand notation of the physicists.

Perhaps one wishes to denote  $\psi(x)$  as a 'state' or 'state function' in order to express the idea that a given  $\psi(x)$  not only determines the probabilities of various  $x$ -values displayed in  $x$ -tests, but also the probabilities of various  $C$ -values in  $C$ -tests, and  $D$ -values in  $D$ -tests, and so forth. If this were so, i.e. if the given function  $\psi(x)$  actually represented definite probability links with *all* states, then this function  $\psi(x)$  would indeed deserve its name as a state-function, in agreement with the idea that every (well defined) state has definite probability links with *all* other (well defined) states. However,  $\psi(x)$  does not have this supposed quality. Upon the question: how large is the probability 'in the state  $\psi(x)$ ' of finding the state  $C_m$  in a  $C$ -test, the superposition rule answers: it is the absolute square of the amplitude.

$$\psi_{km} = \int \psi_k(x) \overline{\psi_m(x)} dx \quad (\text{for } \sum_x \psi_{kx} \psi_{xm}). \quad (15)$$

Here we have attached a subscript  $k$  which may be omitted only if one prefers an inaccurate notation. (The bar over the second factor indicates the complex conjugate.) At any rate, the first factor under the integral is our 'state function'. The second factor, however, is quite another 'state function' unrelated to the first. Thus the given 'state function'  $\psi(x)$  or  $\psi_k(x)$  or  $\psi_{kx}$  does not contain information about the probability of other states  $C_m$  turning up in  $C$ -tests, but solely information about various  $x$ -values turning up in  $x$ -tests. Of course, one may take it for granted that all other functions  $\psi_m(x)$  are also known, so that the  $\psi_{km}$  on the right of (15) is determined. Saying



that  $\psi_k(x)$  or  $\psi_{kx}$  determines  $\psi_{km}$  where  $m$  or  $C_m$  represents any state whatsoever, would be equivalent to saying that Canadians can translate from English to French (from  $k$  to  $x$ ) which is correct, but that they also can translate into *all* other languages (from  $k$  to *all*  $m$ ), taking it for granted that they have all the dictionaries (from  $x$  to *all*  $m$ ), which is misleading to say the least. Such misunderstandings have indeed misled many of those working on quantum theory. It is preferable to hold on to the established terminology according to which a *state* is defined by a datum or combination of data obtained from meters. Such a (well-defined) state is connected with *all* other (well-defined) states by  $\psi$ -links. A function  $\psi_k(x)$  or  $\psi_{kx}$  describes the link between one state  $k$  or  $A_k$  with the one orthogonal set of positional states  $x$ . A function  $\psi(x)$ , which anybody can write down at random, does not represent anything physical.

### (g) *Identical Particles*

Another topic belonging to the general probability metric, independent of the dynamical constant  $h$  is the predilection of identical particles for either crowding together in states in an 'unclassical' fashion (Bose) or pushing out one another from the same state (Pauli, Fermi). These two modes of interaction are represented by  $\psi$ -functions which are symmetric and/or antisymmetric, with respect to the permutation of identical particles. However, the restriction of  $\psi$ -functions to symmetry (hence Bose statistics) or antisymmetry (hence Fermi statistics) is nothing to be surprised about, since these two types are the only types of  $\psi$ -functions which satisfy the requirement that all *observed* qualities pertaining to the system of identical particles, among them the probabilities  $p = |\psi|^2$  are *symmetrical*, i.e. remain unchanged under any permutation of the identical particles. In short, the symmetric (Bose) and the antisymmetric (Pauli-Fermi) modes of interaction are the only ones which provide for *indiscernibility of identical particles* in every observational respect. But this is a requirement so self-evident that it must be classed under those 'laws' of physics which are mere definitions—supplemented, however, by the empirical statement that there *are* indeed uncounted millions of indiscernible particles.

Both the symmetric and the antisymmetric modes of interaction imply that the particles at one end of a long molecule 'know' what all the others are doing, in order to do the same as much as possible (Bose) or not doing the same (Pauli). This mutual communication

over long distances is often considered as bordering on the miraculous by those who forget that the 'long' distances are still so short that one still has practically instantaneous interaction. (All this refers to non-relativistic theory.) 'There seems to be some aspect of nature of great philosophical content beyond the understanding of the physicist. He must be content to accept the implications without hoping to penetrate the mystery that is implied.'<sup>1</sup> But why this awe before the inscrutable? It would rather be an aspect of great philosophical content if identical particles were discernible. There is no *special* mystery in the fact that  $\psi$ -functions are symmetric or anti-symmetric with respect to identical particles.  $\psi$ -functions may seem mysterious as long as they are taken as manifestations of certain 'quantum principles' of cryptic origin. But we have seen that the probability structure of interrelation between orthogonal sets of states does not require any separate quantum principles but follows from entropy-continuity, and that the introduction of directed vectors  $\psi$  between states is a convenient notation for expressing 'orthogonal transformation' between orthogonal sets of states, rather than presenting a new independent law of nature.

It is true that one cannot yet explain why electrons and other particles of spin  $\frac{1}{2}h$  prefer the antisymmetric Pauli way of interaction; under the principle of identity of indiscernibles they might as well choose the Bose way. But this is not an inscrutable mystery; it is connected<sup>2</sup> with other unsolved problems of the relativistic quantum theory of matter and fields. The present report deals exclusively with the non-relativistic theory.

The Nernst theorem (Third Law) is connected with the symmetry or antisymmetry of the  $\psi$ -functions of  $N$  identical particles forming a body. Every state of such a body could be realised by  $N!$   $\psi$ -functions leading to an  $N!$  fold 'degeneracy', except for the symmetry requirement; this reduces the number  $N!$  to only *one* permitted  $\psi$ -function, either symmetric or antisymmetric, and thus reduces the degeneracy to non-degeneracy.<sup>3</sup>

The Nernst theorem must not be confused with the simple statement that one can never reach the absolute zero point  $T = 0$ , because this empirical looking statement becomes self-evident when one re-

<sup>1</sup> Sir John Lennard-Jones, 'New Ideas in Chemistry', *Scientific Monthly*, 1955, 80, 175

<sup>2</sup> W. Pauli, *Neils Bohr and the Development of Physics*, London, 1955

<sup>3</sup> E. Schrödinger, *Statistical Thermodynamics*, Cambridge, 1946

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places the artificial absolute temperature scale  $T$  by the more natural scale  $\tau = \log T$  where  $\tau = -\infty$  for  $T=0$ ; and  $\tau = -\infty$  can obviously never be reached. Chasing the absolute zero point is the Achilles chase in reverse. Achilles of course *reaches* the tortoise; only the trick of dividing a finite interval into an infinite number of steps makes it appear a surprising empirical fact that he actually succeeds.<sup>1</sup> In contrast, the physicist of course *cannot reach*  $\tau = -\infty$ ; only the trick of condensing the infinite  $\tau$ -interval into a finite  $T$ -interval makes it appear to be a surprising empirical fact that he actually will *not* succeed in reaching his goal. Considering the unattainability of  $T=0$  as a new law of thermodynamics is as misleading as regarding the fact that Achilles catches up with the tortoise as a new law of kinematics. Actually Nernst got the Nobel prize in physics for a physical discovery about bodies approaching the (obviously unattainable) limit  $T=0$ , namely, that the entropies of various states of association and aggregation of particles forming a body converge toward a *common* entropy value differing by *finite* amounts from the various entropies at higher temperatures. This is a very far-reaching empirical statement. Calling it the Third Law of thermodynamics is perhaps not quite justified, since it is a consequence of quantum statistics, which itself is a consequence of the general probability metric (applied to identical particles) and which in turn is a consequence of the fundamental theorem of *thermodynamic continuity*, an amendment to the Second Law.

### 2 Quantum Dynamics

#### (h) Quantum Rules of Periodicity

The general theory of transition probabilities between states of a mechanical system, including the  $\psi$ -superposition rule, is usually counted as part of the quantum theory, although it does not make any reference to the characteristic constant  $h$  of Planck. The latter dominates the relation between special observables, namely the 'conjugate' co-ordinates  $q$  and momenta  $p$ . In classical mechanics 'conjugacy' is defined in terms of the canonical equations of motion. But this definition fails when there are no deterministic equations of motion. Quantum theory recognises conjugates as quantities which are in a *periodic* relation. The older quantum theory simply decreed

<sup>1</sup> Gilbert Ryle, *Dilemmas*, Cambridge, 1954, and A. Grünbaum, *Scientific Monthly*, 1955, **81**, 234



$p = h/\lambda$  where  $\lambda$  is a periodicity in space, the wave-length; and  $E = h/\tau$  where  $\tau$  is the period in time, the reciprocal of the frequency  $\nu$ . These older quantum rules have been replaced by those of Born

$$qp - pq = h/2i\pi \text{ (commutation rule)} \quad (16)$$

and Schrödinger

$$p = (h/2i\pi)d/dq \text{ (operator rule).} \quad (17)$$

Both are equivalent to the result

$$\psi(q, p) = \exp(2i\pi qp/h) \quad (18)$$

which is a periodic function in  $p$  for a fixed value of  $q$ , and a periodic function in  $q$  for a fixed value of  $p$ . All three are *assumptions* introduced in order to fit the observed facts. Shall we accept them simply because they 'work', or could they perhaps be understood as consequences of more fundamental and simple principles? The most abstruse answers have been given to this question. For more than twenty-five years one has considered the wave function (18) to be the expression of a dominant principle of duality, of a fundamental plan of nature to conceal forever whether matter really consists of particles or of waves. One philosopher-poet has exalted the alleged duality as the juxtaposition of the continuous and the discontinuous, as the eternal duality of the male and female element pervading all nature.

In the first place it must be said that quantum theory does not involve a conflict between a wave and a particle nature of matter at all. Matter still consists of particles, and of particles alone, as its *substance*. These particles may display a variety of *qualities* under various observational conditions. One among them is the probability of location in various points  $q$ . How large this probability is depends on the state ascertained in the test preceding the position test. If the preceding test was a momentum test yielding a certain value  $p$ , then and only then is the probability amplitude  $\psi(p, q)$  of encountering locations  $q$  of the form (18), i.e. periodic with wave-length  $\lambda = h/p$ . If this periodic feature of one single property of a particle is the famous *duality*, then swans must be rather dualistic birds since on the one hand they *are* individuals or particles, on the other hand they *have* the quality of wavy necks, so that 'one will for ever remain uncertain whether swans are really particles or waves'. This duality is a contrast of incontrastibles.

Another version of duality wrongly asserts that some phenomena of atomic physics can be explained *only* in terms of the wave-function, others only in particle fashion. Thus, the maxima and minima of

intensity in experiments of diffraction of electronic rays by crystals are said to suggest that matter consists of waves, whereas the Compton effect is said to show an energy and momentum exchange between photons and electronic particles. To quote an otherwise excellent textbook : ' electrons, instead of having laws similar to classical laws, obey the laws of wave motion . . . and light is corpuscular in nature, at least when it interacts with matter '. All this looks like wave theory on Monday, Wednesday, Friday and particle theory the rest of the week (Bragg).

Nothing could be further from the truth. Those interference maxima and minima which for a hundred years were considered as a conclusive proof of the wave-theory of light, and recently also of matter, can, in fact, *also* be interpreted as resulting from an exchange of energy and momentum between the incident particles and a diffraction grating or space-periodic crystal. On the other hand, the Compton effect, usually interpreted as an exchange of energy and momentum between particles, can *also* be explained in wave fashion without using the concepts of energy and momentum at all.

To be more specific, a *crystal* when tested by a position meter turns out to be a periodic arrangement of matter in space. When tested by a momentum meter the same crystal turns out to be a mechanical system which transmits selected 'quantised' amounts of momentum. The two just mentioned 'complementary' test methods revealing space distribution and momentum activities of the same crystal are only two among an infinite number of possible test methods with various types of meters. Any two states which may be revealed in tests are linked by a directed vector  $\psi$ , usually written in the complex notation  $|\psi|e^{i\phi}$ . But only two data  $q$  and  $p$  are linked by a  $\psi$  which as a function of  $q$  is periodic at fixed  $p$ , and as a function of  $p$  is periodic at fixed  $q$ . Disregarding all the other  $\psi$ 's is not justified from a more general point of view.

When the author a few years ago wrote a textbook<sup>1</sup> on quantum mechanics he thought that the first step toward a clarification of duality ought to be the demonstration in a number of examples that there is no contrast between an alleged wave- and particle-nature of matter but only a contrast between various properties of particles, and of particles alone, revealed in different types of tests. Today he considers such examples as quite instructive for clarifying the worst misconceptions of the myth of duality. But he also thinks that the

<sup>1</sup> A. Landé, *Quantum Mechanics*, London, 1951

easiest rational approach to quantum theory is that of first developing the general probability metric on the basis of thermodynamic continuity, and introducing the periodic relationship between conjugate observables in a separate chapter on dynamics, as proposed here.

(i) *Explanation of the Quantum Rules*

When Max Planck in 1900 found his oscillator rule,  $E = h\nu$ , physicists asked for an explanation of this strange quantum fact. Today we have the newer and better quantum rules of Born and Schrödinger; but it seems that now the interest in finding an explanation on grounds of simple and general postulates has died down. The quantum prescriptions have become articles of faith, repeating the history of Newton's law of gravitation. And yet it is possible to *deduce* the quantum rules from the general probability metric combined with a property of conjugates well known from classical dynamics. We are referring to the result: The probability of a system dwelling in a certain domain of phase space ( $qp$ -space) is proportional to the volume of this domain, in short, *the probability density in phase space is constant*. This result is of great generality since it does not depend on the special energy function of the mechanical system, but applies to any system whatsoever. It is plausible that this general *statistical* result of classical mechanics should also be the backbone of the new statistical theory known as quantum mechanics, and constitute the basic element of *correspondence* between classical and quantum theory. This correspondence is simpler than that of Bohr's and more fundamental than Dirac's correspondence between classical Poisson brackets and quantum product differences  $qp - pq = h/2i\pi$  since the latter is already one of the quantum rules.

We thus have the following elements of the new mechanics:

- (I) The general probability metric of transition probabilities, also manifested in the  $\psi$ -superposition rule, and resting on the postulate of thermodynamic continuity,
- (II) the postulate that conjugates  $q$  and  $p$  (position and momentum) have a constant probability-density in phase space (i.e.  $qp$ -space or  $pq$ -space), taken from statistical classical mechanics as the basic element of correspondence.

Combination of (I) and (II) leads by simple mathematical reasoning to the consequence<sup>1</sup> that the amplitude function  $\psi(q, p)$  must have

<sup>1</sup> A. Landé, ' $\psi$ -Superposition and Quantum Rules', *Am. J. Phys.* and *J. de Physique* (forthcoming)



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the form of a periodic function  $\exp(iqp/\text{const})$ , in which the constant has the now familiar name  $h/2\pi$ , as in (18). But (18) leads to the quantum prescriptions of Born and Schrödinger, to Heisenberg's uncertainty relation, to Bohr's complementarity, and all the rest. If these matters were once beyond our understanding, if they were 'riddles', then they have now been solved by reduction to the easily understandable, if not intuitively obvious.

But one should realise that possession of the formal structure of quantum theory does not solve the greater riddle of the constitution of matter itself with its various elementary particles and their interaction, transformation, creation, annihilation, etc. It may be that a relativistic quantum theory together with the introduction of an elementary length will solve these riddles. For the time being, non-relativistic quantum theory can be seen as a coherent structure, based on postulates of thermodynamic *continuity* and mechanical *conjugacy* as steps in the direction of 'removing any notion of miracles from purely natural actions and making things run in an intelligible manner' (Leibniz).

Among the questions dealt with in this report are the following :

(1) Why are there discontinuous transitions between states controlled by probability, and is there a chance of eventually reducing them to deterministic causes ?

*Answer :* Discontinuous events controlled by statistical laws are a consequence of the postulate that there is a continuity of fractional equalities between equality and inequality, i.e. between inseparability and total separability. This postulate is *required* in order to overcome the Gibbs discontinuity paradox of the entropy. It *leads* to a general metric of transition probabilities.

(2) Does the introduction of complex probability amplitudes  $\psi$  and their connection by a superposition rule constitute a new independent empirical 'law of nature' ?

*Answer :* It does not.  $\psi$ -superposition is nothing but an elegant formulation of the metric relations between real probabilities. It is analogous to the introduction of vectors and their addition in geometry.

(3) Why do identical particles either crowd together or exclude one another from the same state in an unclassical fashion (Bose or Pauli-Fermi interaction) ?

*Answer :* Not because of a mystic telepathy but because  $\psi$ -superposition together with identity of indiscernibles leave no other choice than the two quantum ways of interaction, either with symmetric or antisymmetric  $\psi$ -functions.

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### (4) Does physics define a direction of time ?

*Answer :* Classical mechanics even with addition of statistical hypotheses does not give any preference to the future over the past. Quantum theory, which is a theory of states ascertained in tests, yields a monotonic development of an ensemble of systems toward a larger entropy in test after test. On the other hand, the record of the test results shows which test was earlier and which was later. Classical records would not show such an order at all ; but the classical theory of inspection without infringement is untenable.

(5) How can this time direction be reconciled with the Schrödinger time-dependent differential equation in which  $+t$  can be replaced by  $-t$  ?

*Answer :* The Schrödinger equation is not a process equation. It only tells us what *would* be the statistical situation at time  $t_1$ , before or after  $t_0$ , if the statistical situation at  $t_0$  were known to a higher degree than any experiment at  $t_0$  could ascertain (namely,  $\psi$ -phases), yet forbidding us to make any test at  $t_0$  at all, since this would create a new situation invalidating the result expected according to the Schrödinger equation at  $t_1$ .

(6) Why are there periodic relations such as  $E = h\nu$  and  $p = h/\lambda$  and the modern quantum prescriptions of Born and Schrödinger ?

*Answer :* The quantum periodicity rules are consequences of the general probability metric ( $\psi$ -superposition) applied to conjugate quantities  $q$  and  $p$  when the latter are defined by virtue of the constant probability density of  $qp$ -space, a quality known from classical statistical mechanics.

### (7) What is duality ?

*Answer :* Duality is an illogical contrast of incontrastibles, namely, of an object (particle) and *one* of its many qualities. It is not true that some phenomena require a wave-theory and others a particle-theory for their explanation. Duality was introduced originally as a 'principle' for the purpose of canonising the quantum rules of periodicity.

### (8) What is complementarity ?

*Answer :* Complementarity points out the limitations of common ideas about physical reality, in particular concerning conjugate observables. Going beyond this negative result, a positive approach to quantum theory can be obtained through the principle of entropy continuity combined with the statistical quality of conjugate observables mentioned in question (6).

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# ETHOLOGICAL MODELS AND THE CONCEPT OF 'DRIVE' ★

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## I *Introduction*

IN recent years considerable advances in the study of animal behaviour have been made possible by the work of Lorenz,<sup>1</sup> Tinbergen<sup>2</sup> and others influenced by them. Their ethological<sup>3</sup> approach to behaviour study has been fertile partly because it has not been hobbled by an over-rigid theoretical scheme. This has allowed due attention to be paid to the initial descriptive phase, essential in the development of any natural science, but neglected so often by behaviour students; and has also ensured that the explanatory concepts have been chosen to suit the phenomena studied, rather than vice versa. It does not seem over-optimistic to suggest that ethology is now entering a period of rapid expansion—a process which may, however, require a thorough revision of some of the concepts which have grown up with it and seen it through its teething troubles. The purpose of this paper is to consider the models used hitherto by ethologists in the light of some of the known properties of scientific models, and to elucidate their relationship to the ethologist's conception of 'drive'.

The two models concerned are Lorenz's 'Hydraulic reservoir'<sup>4</sup> and Tinbergen's 'Hierarchical system of centres'.<sup>5</sup> Lorenz postulates for each instinctive act, a particular 'reaction specific energy' which he pictures as accumulating in a reservoir with a spring-valve at its base. In an appropriate stimulus situation, the spring-valve is released partly by the hydrostatic pressure of the reservoir's contents and partly by the action of the external stimulus, which is pictured as

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<sup>1</sup> K. Lorenz, *J. f. Ornithologie*, 1935, 137-213, 289-413

<sup>2</sup> N. Tinbergen, *The Study of Instinct*, Oxford, 1951

<sup>3</sup> Lorenz, *op. cit.*; Tinbergen, *op. cit.*, W. H. Thorpe, *Nature*, 1954, 174, 101; R. A. Hinde, in press in American Psych. Soc. study of the status and development of psychology; K. Lorenz, *Sym. Soc. Exp. Biol.*, 1950, 4, 221.

<sup>4</sup> Lorenz, *op. cit.*

<sup>5</sup> Tinbergen, *op. cit.*



a weight on a scale pan pulling against the spring. Tinbergen, considering the total behaviour of the animal, uses a basically similar analogy when he speaks of 'motivational impulses' accumulating in 'nervous centres' where they are held in check by a 'block' which can be removed by an 'innate releasing mechanism' responsive to particular external stimuli. The 'centres' are supposed to be arranged in hierarchical systems each of which constitutes an 'instinct'.

Lorenz's model has been successful in accounting for many features of instinctive behaviour. For instance many instinctive responses are less easily evoked just after they have been performed—this is pictured by the emptying of the reservoir and the consequent reduction in hydrostatic pressure on the valve, so that a stronger external stimulus (weight on the scale pan) is necessary to release it again. Similarly the gradual increase in responsiveness with time since the previous performance is pictured as the gradual refilling of the reservoir. Tinbergen's scheme provides in addition an opportunity for comprehending the organisation of large sections of the animal's behaviour, and indicates the relations between the causal factors which govern it. These two models have thus played a fundamental rôle in the important recent advances in the study of animal behaviour for which Lorenz and Tinbergen have been mainly responsible.

## 2 *Some properties of models*

The models or analogies used in the study of behaviour range between two extremes—those which resemble very closely that which they represent, and those (the 'as if' type) which are extremely remote from it. Lorenz's hydraulic analogy is clearly of the latter type—he referred to it as 'a hydro-mechanic model which, in spite of its extreme crudeness and simplicity is able to symbolise a surprising wealth of facts really encountered in the reactions of animals'. Tinbergen's hierarchical scheme, on the other hand, was closer to the former type—while recognising its 'provisional and hypothetical nature', he described it as a 'graphic picture of the nervous mechanisms involved'.

'As if' models may be of great value for explanation and exposition, and for suggesting predictions and hypotheses: they may even lead to advances beyond those possible on a strictly rational (in a narrow sense) level. Further, there is little danger of forgetting that they are

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only analogies—the properties of the model are not inadvertently ascribed to the original.<sup>1</sup>

On the other hand, MacCorquodale and Meehl,<sup>2</sup> discussing the use of such models in psychology, argue that it is their business to be 'true'—they must have 'some probability of being in correspondence with the actual events underlying the behaviour phenomena'. They believe this requirement is particularly urgent because some day neurophysiology will catch up with behaviour theory: the constructs employed must therefore be compatible with neurophysiology.

Here, however, lies a dilemma. As Arber points out, the very essence of an analogy is imperfection. When a model becomes perfect, it ceases to be a model and loses its *raison d'être*. It is only because it differs from the original that it is useful—because of its simplicity it can pose questions, suggest relations or be manipulated in a way in which the original cannot. On the other hand, the closer a model approaches the original, the more likely are the questions which it suggests to be relevant. Thus while a too good model is infertile, a too remote one is misleading.

There is, however, another danger which becomes greater as a model approximates more closely to the original. It is easy to assume that *all* the properties of the model exist also in the original, and to confuse the two in arguments in which the model is employed.<sup>3</sup> This danger is especially great when 'close' analogies develop out of 'as if' ones. Thus Tinbergen's scheme, while containing concepts like 'nervous centres' which are neurophysiologically possible, has also carried over from the Lorenzian reservoir 'motivational impulses' which, as Tinbergen uses it, is a neurophysiologically improbable concept. It has been argued elsewhere that, immensely valuable as Tinbergen's scheme is in many respects, a scheme involving motivational impulses flowing down a hierarchical system of 'centres' and subsequently being discharged in action involves at least three pre-suppositions which are in fact incompatible with the data.<sup>4</sup> These are:

(i) The causal relations in such a scheme are one-way only—from above downwards. There is thus no place for feed-back stimuli

<sup>1</sup> A. Arber, *The Mind and the Eye*, Cambridge, 1954, pp. 40-44; K. J. W. Craik, *The Nature of Explanation*, Cambridge, 1943, p. 51; S. E. Toulmin, *The Philosophy of Science*, London, 1953, pp. 31-39 and 165-167

<sup>2</sup> K. MacCorquodale and P. E. Meehl, *Psych. Rev.*, 1948, **55**, 95

<sup>3</sup> R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953; M. B. Hesse, *This Journal*, 1953, **4**, 213

<sup>4</sup> R. A. Hinde, *Brit. J. Anim. Behav.*, 1954, **2**, 41

which result from the activity and influence the 'higher centres'. Such stimuli are now known to play an essential rôle in the control of behaviour.

(ii) A lowering of responsiveness is represented by the discharge of motivational impulses in motor activity—whereas in practice it may be due solely to the perception of stimuli (see section 3).

(iii) Each centre is prevented from discharging motivational impulses continuously by a block, pictured as being just beneath it. This is discussed in section 5.

This danger of attributing properties of the model to the original can be avoided only by making the properties of the model quite explicit, and discriminating between those known to 'correspond' to the original and those which are being used to pose questions about it. Through lack of these precautions the hydraulic properties of the old reaction specific energy have invaded many ethological concepts. Although fertile in its day, the continued surreptitious influence of this analogy may divert attention from relevant questions.

### 3 *The ethological concept of 'drive'*

Some concept of drive is essential for any system of behaviour study, but the term itself has had a complex history,<sup>1</sup> and has frequently led to ambiguity and confusion. In this, ethology is no exception, for the term 'drive' has been used in at least three different ways. So far this has mattered little, for authors have usually stated the sense in which they are using the term. Indeed some flexibility in the concepts used is desirable at the present stage in ethology: over-rigid ones would both narrow and stultify research. It is, however, worth while examining these uses here, for in addition to this ambiguity the term often conceals a reference to the hydraulic model.

*Drive: Sense (1).* Drive refers to a central state 'caused' by hormones, stimuli, etc., which itself 'causes' the animal to behave in a particular way. Thus Tinbergen<sup>2</sup> writes 'These internal factors, sometimes together with external stimuli, activate a "drive" . . . An activated drive causes specific searching behavior.'

The ways in which ethologists (the present writer included) have used 'drive' in this sense often indicate the presence of hidden 'existence postulates' which are at least irrelevant, and often mis-

<sup>1</sup> Thorpe, op. cit. ; W. H. Thorpe, *Bull. Anim. Behav.*, 1951, 9, 41

<sup>2</sup> N. Tinbergen, *Quart. Rev. Biol.*, 1952, 27, 1



leading. Thus when the drive is spoken of as 'flowing', 'finding an outlet', having 'impetus', being 'expended', etc., something with similar (hydraulic or electric?) properties to the hypothesised 'reaction specific energy' is somewhere in the background. The use of terms such as 'discharged', 'thwarted' and 'sparking over' also seem to imply such a model. Thorpe, who uses 'internal drive' in this context, is clear about this, using it as a synonym for 'reaction specific energy'—'Lorenz's idea of internal drive held back by the block or inhibiting action of the innate releasing mechanism. . . .'<sup>1</sup>

When a drive is spoken of as being 'activated', on the other hand, the term seems to refer to a mechanism: the terms 'motivated' or 'motivation' are often used to indicate the 'state of activation'. Thus Tinbergen<sup>2</sup> writes: 'As an equivalent of the lengthy expression "the sexual drive of the animal is activated", I will say "the animal is sexually motivated".' Elsewhere, however, he uses 'drive' to refer to the state, and refers to the mechanism as an 'instinct'—'there is a surplus of drive, due to the simultaneous activation of incompatible instincts'.

In such cases 'drive', used to describe something which 'flows' and is 'discharged' and which is 'activated', seems to fall into a class with the word 'fire', which 'burns', is 'put out' and can be 'lighted'. However, although the ways in which the term has been used by investigators may have important implications about the nature of the underlying phenomena, a close reading of the literature makes any attempt to tie down all the implications of the term 'drive' as used by ethologists a hopeless task. How is one to form a picture of an entity which, in addition to being 'discharged', 'thwarted', etc., may be 'satisfied' or 'unsatisfied', is 'consumed by' movements which are 'fed' by it, which is 'obeyed' by the animal, may or may not be 'generalised', has appetitive behaviour 'belonging' to it, may 'conflict' or 'compete' with other drives, and may even 'lack co-ordination' with the 'cognitive element'?

Such uses involve not only hidden existence postulates<sup>3</sup> of a distracting type, but also a confusion between the 'as if' Lorenzian model and the 'close' Tinbergen one. Only when these hidden postulates are made explicit will it be possible to decide whether it is relevant to ask where drives come from or go to, under what conditions they overflow, and so on, or whether these questions are simply meaningless.

<sup>1</sup> Thorpe, *op. cit.*    <sup>2</sup> Tinbergen, *op. cit.*    <sup>3</sup> MacCorquodale and Meehl, *op. cit.*

This may be exemplified by the question of 'consummatory stimuli'. The hydraulic model of drive portrays not only 'what happens' *between* stimulus and response, but also the relations between the response and the (usually) associated reduction in responsiveness—the drive both 'impels' action and is 'discharged' in action, and the two seem inevitably linked. In practice, of course, they are not—the reduction in responsiveness may be a consequence not *directly* of the response, but of the consummatory stimuli which the animal thereby encounters. For instance, the cessation of eating in dogs is governed by stimuli which the animal receives as a consequence of eating—such as stimuli resulting from food in the stomach.<sup>1</sup> The hydraulic model obscured this possibility.<sup>2</sup>

Further similar examples will be given later, and suggest that there may be a warning in the similarities between the ways in which 'drive' and 'fire' are used. 'Reaction specific energy' and 'motivational impulses' may prove as elusive as 'phlogiston'—a concept which was useful in its day, but hindered research when unduly reified.

*Drive : Sense (2).* Here the term refers to all causal factors other than those received through exteroceptors. Thus Moynihan<sup>3</sup> defines it as 'specific readiness to respond to releasing stimuli'.

*Drive : Sense (3).* Recently 'drive' has come to be used for *all* the causal factors influencing the behaviour, whether internal or external. Thus Thorpe<sup>4</sup> defines it as 'The complex of internal and external states and stimuli leading to a given behaviour'. This is similar to an older use of Tinbergen's<sup>5</sup> where 'drive' was referred to as 'the complex of factors determining mood'. This usage has been especially fertile in the initial analysis of complex types of behaviour like aggression and courtship, where the animal has simultaneous tendencies to behave in incompatible ways, for it can be used for the complex of factors on which the tendencies depend. In practice, however, Sense (2) and Sense (3) have often concealed hidden references to the hydraulic model in the same way as Sense (1) : this is especially a *surd* in the case of Sense (3) where the 'drive' includes external stimuli which can in no sense be said to 'flow'.

<sup>1</sup> H. D. Janowitz and M. I. Grossman, *Amer. J. Physiol.*, 1949, **159**, 143-148

<sup>2</sup> Thorpe, *Nature*, as cited ; Hinde, *op. cit.* ; M. Bastock, D. E. Morris and M. Moynihan, *Behaviour*, 1953, **6**, 66      <sup>3</sup> M. Moynihan, *Behaviour*, 1953, **5**, 58

<sup>4</sup> W. H. Thorpe, *Bull. Anim. Behav.*, 1951, **9**, 34

<sup>5</sup> N. Tinbergen, *Bibl. Biotheoret.*, 1942, **1**, 2

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There is a further important ambiguity concealed in all three senses of the term, and most especially in the last two. A definition in terms of, say, 'states and stimuli leading to a given behaviour' is satisfactory just so long as any one set of states and stimuli always leads to the same behaviour—or, strictly, to an instance of the same limited response class—and vice versa. This, of course, is not always the case. Thus when an animal has been unable to show a particular behaviour pattern because it has been deprived of the appropriate releasing stimulus, the behaviour may be shown to a normally quite inadequate object, or even *in vacuo* (*Leerlaufreaktion*).<sup>1</sup> Here the 'drive' does not include an external stimulus which is present when the same response is given on another occasion. Further, when a change in behaviour occurs as the result of a learning process, such as habituation, are we to say that there has been a change in drive? The phrase 'states and stimuli' is sufficiently broad for a learning process to be included. But is this what is intended? And would not this make the ethological use of 'drive' even more different from that in other contemporary approaches to the study of behaviour?

We may now consider two other points where the hydraulic model still exerts a hidden influence which may distract attention from relevant problems.

### 4 *Displacement activities*

Tinbergen<sup>2</sup> defines a displacement activity as 'an activity belonging to the executive motor pattern of an instinct other than the instinct activated'. It is thus defined in terms of the context of the behaviour pattern in question—a pattern developed (phylogenetically or ontogenetically) in one context appears in another apparently 'irrelevant' one. (A decision as to 'relevancy' may of course involve difficulties, but these will not be discussed here.) In a recent paper Tinbergen<sup>3</sup> is quite clear about this and insists that the irrelevance is a matter of fact, while the presence of excess of 'drive', which often seems to be associated with displacement activities, is a *hypothesis* about causation. However, the concept of displacement activities has been so closely associated with hydraulic models that it is often assumed that they necessarily involve the transference of flowing 'energy' or 'motivational impulses' from one channel to another. Thus the present

<sup>1</sup> Lorenz, *Sym. Soc. Exp. Biol.*, as cited

<sup>2</sup> N. Tinbergen, *Quart. Rev. Biol.*, 1952, 27, 1

<sup>3</sup> Tinbergen, *op. cit.*



writer has thought of displacement activities as apparently involving 'a dissipation of the energy through a totally different channel'.<sup>1</sup> Similarly Armstrong<sup>2</sup> states in his formal definition that displacement activities occur 'as a consequence of tension and the resultant deflexion of energy', thereby confusing definition and attempted 'explanation'.

In this paper Armstrong is at least explicit about how he is using his energy model. In a recent paper from another source,<sup>3</sup> which gives a valuable review of displacement activities and related types of behaviour and raises a number of important points, displacement activities are discussed in terms of 'sparking over'. The way in which this phrase is used, however, makes it extremely difficult to understand the nature of the model used. Sometimes it is the 'nervous energy', sometimes the 'impulses', sometimes the 'behaviour' (e.g. incubation), sometimes the 'nervous system' and sometimes even the 'fish' which is said to 'spark over'.

### 5 *Innate releasing mechanism*

This is a hypothetical construct first introduced to account for the observation that an animal responds to only a limited part of the total stimulus situation when making an inborn response. Thus Tinbergen and Perdeck<sup>4</sup> describe it as a mechanism 'selectively responsive to a special stimulus situation'. Baerends<sup>5</sup> incorporates physiological references in his definition: 'The mechanism beginning at the sense organs, ending at the centre released, and including the sensitivity for characteristics of the object, we will call the releasing mechanism'. The concept has, however, accrued a number of other properties: some of these are explicitly stated and are the result of research aimed at elucidating its nature, but others have arisen from preconceived theoretical ideas which have not been recognised as such. Three of these which have been derived from the hydraulic model may be considered here:

(a) Releasing. The use of the term is partly due to the explosive character of some responses, and partly a result of Lorenz's model, in

<sup>1</sup> R. A. Hinde, *Behaviour*, 1952, Supplement No. 2

<sup>2</sup> E. A. Armstrong, *Sym. Soc. Exp. Biol.*, 1950, 4, 361

<sup>3</sup> Bastock *et al.*, *op. cit.*

<sup>4</sup> N. Tinbergen and A. C. Perdeck, *Behaviour*, 1951, 3, 1

<sup>5</sup> G. P. Baerends, *Sym. Soc. Exp. Biol.*, 1950, 4, 337

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which the stimulus 'releases' a valve and allows the 'reaction specific energy' to flow away. However, stimuli whose action may be described as 'stimulating' (which may or may not be basically different from releasing),<sup>1</sup> 'inhibiting' and 'orientating' are also responded to *selectively*, and the I.R.M. is in practice used as a generic term in association with stimuli of all these types.

(b) The block. The I.R.M. is often referred to as incorporating or associated with a 'block'. This is another product of the reservoir analogy, which Tinbergen<sup>2</sup> later rationalised by pointing out that if the various types of motor response were not inhibited for most of the time, the animal would show chaotic movement. Two points must be made here. First, there is no conclusive evidence that anything functionally analogous to such a block exists in the central nervous system. Granted that if you have flowing motivational impulses, something is necessary to stop them flowing; but such impulses are a non-physiological analogy. The non-occurrence of chaotic movement could be conceived in quite different physiological terms; for instance, the efferent discharge could be continuous, but ineffective unless co-ordinated or channelled under the influence of the external stimulus. Second, if there is some sort of block in the nervous system, it need not necessarily be closely associated with the releasing mechanism. The block component of the model *may* eventually turn out to be a 'good' one, but it must be recognised that at present it is a property only of the model, and not necessarily of the original.<sup>3</sup>

(c) Key-lock analogy. The I.R.M. is often compared to a lock which is unlocked by the appropriate 'key' stimuli. This implies a valve-like and essentially static view of the I.R.M.—on Tinbergen's view all I.R.M.s were thought of as equally active all the time. In practice this is not so: with increasing motivation the animal becomes more unselective in the stimuli to which it will respond, even though the intensity of the response, once elicited, does not change. An instance of this was demonstrated by Beach<sup>4</sup> in 1942, but the key-lock analogy caused ethologists to neglect it until Prechtl<sup>5</sup> came across another case.

<sup>1</sup> Hinde, *Brit. J. Anim. Behav.*, as cited

<sup>2</sup> N. Tinbergen, *The Study of Instinct*, Oxford, 1951

<sup>3</sup> Hinde, *Brit. J. Anim. Behav.*, as cited

<sup>4</sup> F. A. Beach, *J. Comp. Psych.*, 1942, **33**, 227

<sup>5</sup> H. F. R. Prechtl, *Behaviour*, 1953, **5**, 32

## 6 Conclusion

We have seen how the use of a particular type of model has influenced ethological concepts ; and how, when the properties of the model were not clearly differentiated from those of the original, this has sometimes had a retarding influence on research. This, of course, is no reflection on the models, which have led to important advances, but does imply that their useful life is limited. But although it is a relatively easy task to point out a few shortcomings in the present models, the substitution of constructive proposals for a new one is far more difficult. Only two further points will be made :

(a) The term drive can be avoided in particular contexts by the use of other terms which do not carry the same ambiguities. Thus, in the analysis of complex types of behaviour, 'tendency' (with an appropriate specification, e.g. 'attacking tendency') is as serviceable, and requires no specialist definition at this stage.<sup>1</sup> Because it is more directly related to the behaviour observed than 'drive', it does not carry the same hidden meanings. Similarly, 'Specific Action Potentiality' (S.A.P.) can be used as a generic term for measures of responsiveness ;<sup>2</sup> and 'internal motivation' for drive in the first sense described above.

(b) It is unlikely that models with hydraulic properties will continue to be useful. For one thing, on Tinbergen's scheme, a causal factor can influence only those activities at a level in the hierarchy lower than that at which it acts. Nevertheless, displacement activities occur. To discuss them in terms of 'sparking over' may be a useful way of describing the behaviour, but it conceals the inability of the hydraulic model to explain how one set of causal factors seems to produce behaviour 'normally' dependent on another set, and gives a spurious appearance of uniformity to a heterogeneous class of activities.

Further, recent work on the 'reticular activating system'—a core of grey matter lying in the brain stem and diencephalon of mammals<sup>3</sup>—indicates that sensory stimuli may have a general effect as well as a specific one. The sensory in-put is relayed via this system to the cortex, as well as proceeding direct ; but within it, though there may be localisation of function, there is no segregation into distinct sensory modalities. Lesions in this region result in a comatose condition, and

<sup>1</sup> R. A. Hinde, *Brit. J. Anim. Behav.*, 1955, **97**, 706

<sup>2</sup> Hinde, *ibid* ; Thorpe, *Bull. Anim. Behav.*, as cited

<sup>3</sup> *Brain Mechanisms and Consciousness* (Sym. of Council Int. Organisations of Medical Sciences), Oxford, 1954



## ETHOLOGICAL MODELS

it may be concerned in functions such as 'alertness'. Although this system has been studied only in higher mammals, there are indications of functionally similar effects in other forms.<sup>1</sup>

Although an extreme form of general drive theory, in which the drive is regarded as a general condition of the nervous system contributed to by all the specific 'needs' of the moment, is unlikely to be accepted by ethologists, some distinction between two modes of action of the factors influencing behaviour—one general, the other specific to a limited group of responses—may be necessitated by facts such as these. This would of course involve a radical departure from any hydraulic 'reaction *specific* energy' type of model.

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<sup>1</sup> G. Birukow, *Verhandlungen der Deutschen Zool. Gesell. in Wilhelmshaven*, 1951, 144

## NOTES AND COMMENTS

### *The Stroboscope as Providing Empirical Confirmation of the Representative Theory of Perception*

It is my purpose in this paper (i) to present certain facts from the field of psychophysiology ; and (ii) to interpret these facts as providing empirical evidence in favour of some form of the representative theory of perception.

(i) *The facts.* The electronic stroboscope is a device in which the discharge from a condenser is passed at regular intervals through a gas vapour tube. This provides a flash of light of extremely short duration (some  $10^{-5}$  seconds). This device has been used for the last decade in electro-encephalography as it was found that the electrical rhythms of the brain can be driven by the flash frequency. During this work it was noticed that when a subject looks at the flashing light certain complicated phenomena develop in his visual field.<sup>1</sup> Grey Walter <sup>2</sup> has discussed certain theoretical aspects of the phenomenon. When the light is flashed at between four and thirty flashes per second he will notice that his whole visual field becomes filled with complex patterns. These are usually composed of a large number of zig-zag dark or coloured lines vibrating with incessant movement upon a light ground. These lines may form a design like the spokes of a wheel, or whirlpool or catherine wheel effects may be observed. The zig-zag lines may sweep from one side of the visual field to the other. Another dominant form of the patterns is a complex mosaic of most intricate structure. Interspersed amongst these line patterns, patterns of coloured dots may be observed. At some frequencies great sheets of brilliant colours may appear and spread over the field. The actual form of the patterns observed will vary very much with the frequency of the flash and with the intensity of the illumination. A most striking feature of the phenomenon is the great complexity of the patterns.

(ii) *The interpretation of the facts.* The questions arise 'How are we to explain this phenomenon and what conclusions of philosophical interest can we draw from it?' Grey Walter <sup>3</sup> has suggested that the observations give us evidence that the physiological mechanisms mediating visual perception of the external world function according to the same mechanical principles as are used in television.<sup>4</sup> His original account should be con-

<sup>1</sup> When a subject looks at the lamp he must either keep his eyes closed or place a sheet of ground glass in front of them to protect his eyes from the strong light.

<sup>2</sup> W. Grey Walter in *Perspectives in Neuropsychiatry*, D. Richter ed., London, 1951

<sup>3</sup> Grey Walter, loc. cit.

<sup>4</sup> It would be more correct to say that television uses the same mechanical principles as are used in the physiological mechanisms mediating visual perception.

## REPRESENTATIVE THEORY OF PERCEPTION

sulted for the full details of his exposition but it may be summarised as follows. The television mechanism has three essential parts—a receptor, a conductor, and a presenter.<sup>1</sup> The spatio-temporal patterns of light and dark shapes (the scene televised) are transformed by the receptor into an electrical pattern of excitation conducted by wires or electromagnetic radiation. The patterns are thus transmitted to the ‘set’—the presenter part of the mechanism. Here the electrical pattern is transformed by a scanning device back into a pattern similar to the original pattern televised, and thus a *copy* of the original scene is *reconstructed* by the mechanism. If the studio is illuminated by a flashing light instead of a steady light similar phenomena to those described above—complex patterns of dots and streaks and lines—will dart and zig-zag across the screen. This will be so because, at each flash, only that part of the screen will be illuminated over which the electron beam is travelling for the duration of the flash. The actual form of the patterns will depend on the particular nature of the scanning mechanism employed : and, therefore, if the *details* of the patterns observed in the case of human vision can be recorded, we may be able to make deductions about the particular nature of the scanning mechanism responsible for them.

Now the following points may be noted : (i) The sensed visual patterns comprising the stroboscopic phenomena are quite flat and two-dimensional. They have no visual depth. They are *spatial* patterns and fill the entire visual field—as far to the right and left, above and below as one can direct one’s attention. The patterns are spatially located, it would seem, about two feet in front of the (sensed) nose. That is to say, they are located where after-images are located.

(ii) Secondly, from the observation of these patterns it is possible to make certain suggestions about the nature of the physiological processes mediating everyday visual perception.

This second point will now be amplified. The stroboscopic phenomena, which may be observed on the screen of the ordinary television set when we interfere in a specific way with the workings of the mechanism, do not arise *de novo* but owe their origin to the fact that the mechanism which presents the ‘normal’ pictures on the television screen is constructed in a particular way. The images we ordinarily see on the screen of the set are themselves constructed by the same scanning mechanism when it is functioning without such interference. Similarly we can argue that the complex patterns we observe when our own physiological mechanisms of perception are subjected to intermittent stimulation do not arise *de novo*, but they suggest that the expanded and coloured visual field as this presents itself to direct observation is itself *constructed* by the physiological mechanisms mediating visual perception (and thus the visual field is

<sup>1</sup> These particular terms are my own.



probably not a structural part or 'direct view' of the 'external world'). For, as we sit in front of the stroboscope with our eyes shut<sup>1</sup> watching the intricate play of the stroboscopic patterns, we can argue as follows:

When my eyes are directed at this flashing light, the patterns that I observe probably indicate that what I actually observe is a (spatial) field in which the events are being immediately determined by a most complex and intricate scanning device. Now when I look away from the lamp at the scene around me illuminated by a steady light I can no longer detect these patterns. My field of direct observation is now filled with (relatively) stable and contiguous patches of colour (sense-data). Nevertheless, if the analogy is valid, this complex pattern of shaped colours must be built up by the individual elements demonstrated by the stroboscopic process. A representative mechanism is designed not to produce curious patterns when the receptor part of the mechanism is illuminated by a flashing light but to construct a *copy* of the scene at which the receptor part of the mechanism is directed. That is to say, that when I look at a scene illuminated with a steady light the position and form and colour of each and every minute element in the whole field is determined by the physiological mechanisms of perception functioning as representative mechanisms constructing a *copy* of the scene at which my receptor sensory organs (eyes) are directed. However, it is also possible that the patterns may originate in the retinal mechanisms and that the perceptive mechanisms central to the retina work wholly by point-to-point projection rather than by scanning. In which case they would still be representative mechanisms but they would use different principles from those used in television. Experimental work conducted in an attempt to determine the origin of the patterns is in progress in this laboratory. It is mechanically impossible for the televisual mechanism to give us a *direct* view of the events televised. The stroboscopic phenomena indicate beyond any reasonable doubt that my perception of scenes illuminated by the stroboscopic lamp—and thus of scenes illuminated by a steady light—is mediated by a representative mechanism. As Grey Walter<sup>2</sup> says, 'The televisual mechanism behaves very much like the neuro-visual one'.

(iii) *Conclusion*. Thus we can suggest that the current variations of naïve realism that are held by some philosophers, in which it is believed that the physiological processes of perception mediate a *direct view* of the physical world, are wrong. Furthermore, we can argue that the only valid theories of perception are those which recognise that the events of which we are directly aware, i.e. the stroboscopic patterns, and thus the visual field of contiguous sense-data itself, are literally *constructed* by the representative mechanisms of perception. These theories are (1) physiological realism (to say with Russell 'our percepts are in our heads'); (2) psychological

<sup>1</sup> The lids function as a red semi-opaque screen.

<sup>2</sup> Grey Walter, loc. cit.

## ON DR. BURNISTON BROWN'S NOTE: A CORRECTION

realism (to say 'our percepts are in our minds'—this presupposes that a part of the physiological or spatio-temporal process of perception is located in the mind); and (3) the 'projection' theory of perception as presented, for instance, by Ruch<sup>1</sup> (he says that our percepts are 'projected' out of our heads into the external world. This presupposes that there are causal processes linking events in our brains with our 'projected' percepts in the external world.) Any choice between these three theories lies outside the scope of this paper.<sup>2</sup> We can only indicate here that the choice must lie between the three.

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### *On Dr Burniston Brown's Note : A Correction*

DR G. BURNISTON BROWN<sup>3</sup> states that I 'define "dimension" to mean the index or power to which each fundamental magnitude occurs in a physical equation'. If he had read my papers<sup>4</sup> carefully, he would have realised, as Dr Focken has done, that I regard the word 'dimensions' in this connection as superfluous, but, to avoid the misunderstanding which might result from an abrupt change of language, I used what I described as 'simply a definition, agreeing with generally acknowledged, but not always followed, convention'.

The correct procedure, I stated, was 'to discard the idea of a physical quantity altogether and to grant meaning only to the process and result of measurement in each case'. The confusion which the redundant word introduces is illustrated by the fact that the *Oxford English Dictionary*, though recording many meanings, gives none that allows an interpretation of such a statement as 'the dimensions of velocity are  $[LT^{-1}]$ ' for instance. The word is indefinable because there is nothing for it to stand for. The so-called 'dimensional symbol' is useful to represent the *process* of measurement, while the symbols in an ordinary physical equation stand for the *results*. I hope, therefore, that Dr Brown will not think that I support or oppose his campaign for his definition.

HERBERT DINGLE

<sup>1</sup> T. C. Ruch in J. F. Fulton, *Textbook of Physiology*, 16th edn., Philadelphia, 1950, p 311

<sup>2</sup> See, however, J. R. Smythies, 'Analysis of Projection', this *Journal*, 1954, 5, 120.

<sup>3</sup> This *Journal*, 1955, 6, 252

<sup>4</sup> *Phil. Mag.*, 1942, 33, 321, 692; 1943, 34, 588; 1944, 35, 296, 588; 1946, 37, 64; 1949, 40, 94

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*Modern Experiments in Telepathy.* By S. G. Soal and F. Bateman.  
Faber and Faber, London, 1954. Pp. xv + 426. 30s.

THIS book performs two functions : it provides a careful survey of the most important experiments in 'extra-sensory perception' carried out in America and Europe during the last forty years ; and it gives a very detailed account of the monumental experiments initiated by Dr Soal. The former function is performed in the first six chapters, and also in many passages in the sequel, where, in the course of describing Soal's results, the authors discuss comparable features in the results of other investigators. In describing the work of others, the authors are concise, accurate, and highly critical. Much, though not all, of the American work fails to come up to their own standards. Chapter VII describes Soal's earliest card-guessing experiments. He set out in 1934 to repeat the early work of Dr J. B. Rhine. In 1939, having tested 160 subjects without finding any extra-chance scoring, he was proclaiming his scepticism about Rhine's claims. Fortunately he was persuaded by Whately Carington to re-examine the records of his 160 subjects to look for the temporal displacement which Carington had recently discovered in a group experiment in telepathy. Soal had matched each of his subjects' guesses only against the card it had been aimed at, the card which the agent (or sender) had been looking at when the subject made his guess (the (O) card). Now he matched each guess against the ensuing (or (+ 1)) card, and the preceding (or (- 1)) card, and he found that two of his subjects, Mr Shackleton and Mrs Stewart, had achieved statistically significant scores in *both* of these categories. With each of these subjects Soal subsequently carried out extensive experiments.

Chapters VIII to XI are devoted to the Shackleton series, involving forty sittings held between 1941 and 1943, conducted with the help of Mrs K. M. Goldney. The most elaborate precautions were taken to rule out possibilities of recording errors, of the subject receiving sensory clues and of the experimenters themselves having opportunities to fake the results. Shackleton succeeded with remarkable consistency in what looked like precognitive telepathy. He averaged about 7 hits per run, instead of the 4.8 expected by chance on the (+ 1) card. When the speed of guessing was doubled his high scoring switched to the (+ 2) card. He was apparently responding to what the agent was going to attend to 2 to 3 seconds after he recorded his guess. When Shackleton was first tested in 1936 he had also made significant scores on the (- 1) card. There was no sign of this in the later series, until, in 1942, the person who had acted as his agent



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in 1936 was reintroduced in this rôle. Working with him Shackleton's scores reverted to the original pattern—high scoring on both the (+ 1) and (− 1) cards (or, at rapid speed, both the (+ 2) and (− 2) cards). Statistically the overall (+ 1) score is enormously significant. Soal gives the odds against chance as  $10^{35}$  to 1, after including all the series in which Shackleton consistently failed; as he did, e.g., with eight of the eleven agents who were tried, and in the 'clairvoyance' conditions, in which the agent did not look at the target-cards.

Chapters XII to XIX are devoted to the experiments with Mrs Stewart, carried out by Soal and Bateman and involving 130 sittings between 1945 and 1950. Mrs Stewart's high-scoring was concentrated throughout on the (O) card, except when the speed of guessing was doubled when her high scoring switched to the (− 1) category. Statistically, the results are even more impressive than Shackleton's: 'on an extremely conservative estimate, the odds must be of the order of  $10^{70}$  to 1' (p. 311). But, as in the work with Shackleton, the experimenters did not aim solely or primarily at amassing huge odds against chance, by sticking to conditions which had proved successful. The conditions were frequently and systematically varied. One of the most interesting variations which proved successful was the 'split agent' experiments (ch. XIII) in which two agents were located in different rooms, each possessing part but only part of the information required to specify which card-symbol was the current target. Important also are the long-distance experiments (ch. XVI) in which Mrs Stewart was in Antwerp 200 miles away from the agent in London. Mrs Stewart continued to score at the same level as when she and the agent were in adjacent rooms. Chapter XIX contains an elaborate analysis of some position effects found in the Stewart data.

The last chapter contains a discussion of some recent criticisms of ESP research. Mr D. H. Rawcliffe's attempt to explain the phenomena in terms of whispering and hyperaesthesia is disposed of without difficulty. Less explicitly formulated, and therefore perhaps more plausible, is the argument of Mr G. Spencer Brown that the phenomena are merely examples of a breakdown in the accepted theory of probability. This argument is effectively met by drawing attention to the many instances in the Shackleton and Stewart experiments in which a change in the experimental conditions, or in the agents, consistently yielded characteristic changes in the nature of the results; and by reference to the 'cross-checks', in which each run of guesses was matched against a run of target-cards for which it was not intended. These consistently yielded chance scores. Why, as the authors put it, should a statistical artefact be a respecter of persons and of experimental conditions? There is a passage in the last chapter in which the authors seem to be arguing in favour of psycho-physical dualism (pp. 338-341), but elsewhere they are usually more cautious, e.g. 'we think it a

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mistake to insist, as so many prominent writers on telepathy have done, that physical radiation theories are impossible' (p. 304). Unlike most writers on this subject, the authors show little inclination to claim that ESP phenomena have metaphysical implications. Theories of pre-cognition are reviewed in chapter X, but the authors conclude that 'a worth-while theory is unlikely to appear until a great deal more experimental work has been done' (p. 174).

Some of the authors' excursions into the realm of theory are open to criticism, e.g. 'apart from memory, the results of psi-tests probably represent the best evidence available that unconscious mental processes do occur' (p. 359). And the definition of 'telepathy' (p. 359) in terms of correspondence between the 'mental patterns' of different people, is liable to mislead, since the guessing of Shackleton and Stewart seems to have consisted in motor responses unaccompanied by mental images, and also it was found unnecessary that the agent should consciously attend to the target-symbols (pp. 211-212). The book is not light reading. Students of parapsychology will be glad that Soal's experiments are reported so fully, for the original report of the Shackleton experiments has long been out of print and the Stewart experiments have not previously been published in full. Some readers may feel, however, that Soal's work is described with unnecessary, and sometimes wearisome detail, which may sometimes make it difficult to see the wood for the trees. The book would have been better balanced if its account of the Stewart experiments had been briefer and simpler, leaving the full details to be published by the S.P.R. Despite such minor defects, this book is likely to become a classic in its field, and the appendices on statistical methods add to its value. No other investigator of ESP has made better use of his opportunities than Dr Soal, either in the rigour of his precautions or in the variety of factors tested. The book is the most thorough and scholarly work on experimental parapsychology now available.

C. W. K. MUNDLE

*Historical Inevitability* (Auguste Comte Memorial Trust Lecture. No. 1).

By Isaiah Berlin.

Geoffrey Cumberlege, Oxford University Press, 1954. Pp. 78. 6s.

WHETHER this work should be called a very long lecture or a fairly short book or a new and unique form of literary art need not concern us here for, whatever we call it, it is undoubtedly learned, stimulating, exuberant, and important. On the other hand it is by no means neat or systematic. It is a philosophical *jardin anglais* in which there are no trim parterres. Certainly, if these long loping paragraphs had been published anonymously no one would have been tempted to attribute them to Professor C. D. Broad.

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A reviewer, therefore, may hope to be of some service by first setting out the main lines of the argument in a somewhat tighter form than is displayed in the work itself.

Perhaps the main purpose of the book is to uphold the independence of the human studies, and in particular of history, from the tyranny of the methods thought to prevail in the predictive natural sciences. But in the course of this operation Mr Berlin finds himself also at war with metaphysical conceptions of history such as the Hegelian or that of Professor Toynbee. Common to these otherwise very different conceptions of history, he holds, is a view of historical explanation as a subsuming of human actions under causal or teleological or supersensible laws. Scientific and metaphysical accounts of human history are thus alike in being deterministic; they agree in holding that what men do depends on the place they occupy in cosmical or social structures; and they both carry with them the implication that there is no such thing as moral freedom, and that moral praise and blame are survivals from a primitive ignorance which civilised men would do well to rid themselves of. It is Mr Berlin's view that, although some of the exponents of historical determinism are (like Condorcet) inspired by benevolent motives, and others (like Marx) indulge in 'sardonic gloating', both alike are trying to resign their responsibility and to find some grand excuse that absolves them from all possibility of blame. That they contradict themselves in entertaining this discreditable wish is part of Mr Berlin's case against them. They fail to realise, he holds, the enormity of the enterprise involved in ceasing to think of human beings in terms of freely made decisions and of the praise and blame they give rise to. Furthermore, they themselves are inconsistent in that they *blame* historians for passing moral judgments in their historical writings. At the root of the relativism which, according to Mr Berlin, is associated with these sorts of determinism, is a false view of objectivity. It is falsely supposed that whereas in the natural sciences there are fixed criteria for distinguishing the true from the false, the criteria used in the ethical sphere are too vague and shifting to merit serious attention from historians and social scientists. In dealing with this Mr Berlin briefly indicates a theory of the categories according to which all are subject to the possibility of change although some (the physical categories of time and space, for example) are more stable than others. He criticises the relativists for their too simple dichotomy between subjective and objective, and argues that there is a gradual diminution in degree of stability from the physical categories through the categories of sensible experience to those of morals, etiquette, and taste. Moralising, therefore, can be well or badly done, so that it is absurd to condemn the very conception of it because it is not always well done. History is not and cannot be a natural science, and those who claim to know what the human future must be are



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false prophets even though they may use the vocabulary of science. This, of course, has already been argued in detail by Professor Popper, and Mr Berlin refers to the 'devastating lucidity' and 'force and precision' of the relevant parts of 'The Poverty of Historicism' and *The Open Society*.

That Mr Berlin's lecture is opportune may be seen from a well known textbook of social anthropology in which it is gravely argued both that all moral valuations are 'ethnocentric' and that recognition of this is good because it promotes toleration. It is opportune, indeed, for more important reasons than this, for the absurdity I have quoted is typical and symptomatic. What is interesting about Mr Berlin's lecture, however, is that it is not only concerned with the inconsistencies in certain false but prevalent views but also with the untenable philosophy that lies behind them. Mr Berlin is an empiricist in that he rejects philosophical views which fly in the face of the facts of experience, but he is not, so it seems to me, a dogmatic empiricist. Dogmatic empiricists accept without question such principles as that all our knowledge is based on experience of sensible particulars. In our own day it is a principle of dogmatic empiricism that science is concerned with facts and philosophy with the language in which facts are reported and discussed. Now it appears from Mr Berlin's criticism of determinism that the composing of this lecture caught him in the very process of emancipating himself from the influence of dogmatic empiricism. In putting his case against determinism he says both that it is incompatible with the distinctions drawn in our ordinary speech and that it is 'too difficult to accept' because it would require us to make unthinkable changes in our conception of the human world. Taken on its own, the first argument would only commend itself to a believer in one form of the 'linguistic' doctrine, but in conjunction with the second it suggests a method not unlike that of Bernard Bosanquet. Again, dogmatic empiricists are always nominalists in that they accept the principle that nothing exists that is not particular. But Mr Berlin is not altogether happy about this principle either. It is true that he makes some quite terrifying passes at those who reify abstractions such as the 'Collectivist Spirit', but he also says that 'collective acts do occur', and concludes his main discussion of the matter with the following words: 'There is no formula which guarantees a successful escape from either the Scylla of populating the world with imaginary powers and dominions, or the Charybdis of reducing everything to the verifiable behaviour of identifiable men and women in precisely denotable places and times. One can do no more than point to the existence of these perils; one must navigate between them as best one can.' This last sentence, I suggest, is unduly modest, and the whole drift of Mr Berlin's most stimulating lecture is to show that there is important work to be done by philosophers who, like him, refuse to oversimplify the complexities of human action.

H. B. ACTON

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*Origin of Life.* By A. I. Oparin. Translated by S. Morgulis.

Dover Publications, New York, 2nd edition, 1953. Pp. 270. \$3.00.

THIS reprint is part of a series and it is by no means clear what policy the publishers are following in choosing books for it. Some of the books, such as Newton's *Opticks* and Galileo's *Dialogues concerning Two New Sciences* are classics and, even in parts where the facts accepted in them have been superseded, they remain valuable as a record of the progress of thought. Others, such as Abbott's *Flatland* and Norman Campbell's *What is Science?* are of interest because of the argument rather than the facts and can compete on an equal footing with present day efforts along similar lines. But Oparin's book is difficult to place. It was very valuable when it appeared in 1937 but it has 'dated' seriously in parts without yet being old enough to have gained historical interest. Those with experience will find re-reading it an interesting excursion into the recent past but it will be a pity if students accept it as authoritative.

The first three chapters set out the problem and summarise the suggestions that have been made about the nature and origin of life from antiquity nearly to the present day. The relative importance of all the people who have written on the subject is clearly a matter of opinion but Oparin seems to attach too little importance to Huxley, Tyndall, and Errera. Many recent books make the same mistake and it is probable that this is because their authors have tended to use Oparin as a guide to the literature instead of studying it for themselves. Apart from this criticism, and within the limits of space, this is as good a survey as has been written.

Two chapters on astronomy, the origin and composition of the earth, and the principles of biochemistry are now of little value. These subjects have moved so rapidly that there are many errors of fact and many of the opinions expressed would not now get general acceptance. Thus most of us assumed, when this book was written, that the earth was, at one time, so hot as to be wholly fluid; but a commonly held view now is that it was built up by the accretion of small cold particles and that the internal heat is secondary. This change of opinion is relevant because it makes the theory that life pervades space, and that it did not originate on the earth but came here on some sort of cosmic detritus, more possible even if it remains highly improbable.

In the next two chapters Oparin explains how a system of immiscible liquids, if it formed at the surface of the primitive earth, would be expected to concentrate various types of complex molecule which would, according to Haldane's well known theory, have been formed under the influence of ultraviolet light. He goes on to show that they would grow and divide when they had reached critical sizes, that a form of natural selection and evolution would probably go on among these droplets and that their complexity would tend to increase. This is much the most valuable part

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of the book. Parts of the argument would probably be presented differently if they were being written now and many readers may not find it wholly convincing, but the essential point, that evolution and selection could operate among systems that no one would now wish to call alive, is brought out clearly. It is Oparin's cardinal contribution to our thinking about the origin of life.

When droplets of the type described are made in the laboratory they have not been shown to have these properties but that does not invalidate the theory. So far they may have been made under the wrong conditions and a search for more relevant conditions would be interesting and perhaps illuminating. It is odd to find Oparin putting considerable stress on the probable small size of these primary units. He compares them with the subunits or organelles that are found in most present day cells and quotes favourably the idea that the first organisms may have been the consequence of a sort of symbiosis between several droplets with different properties. This is plausible enough but its plausibility does not seem to be enhanced by the assumption that the primitive organisms were very small.

There has been an extraordinary accretion of unnecessary assumption about the process of biopoesis and the properties of eobionts—if words proposed recently for the creation of life and the things created may be used (Pirie, *Discovery*, 1953, 14, 238). All that is essential is that Oparin's eobiont should be small enough to hold together until it reached dividing size: whether this size is 10  $m\mu$  or 1 cm. seems to depend on the surface tensions involved and not on the principles of biopoesis.

A further assumption in this book, as in almost all other discussions on biopoesis, is that carbon and nitrogen were the fundamental elements initially as they are now. It may well be that they were but the assumption is at present quite arbitrary. Oparin makes it because he seems to think of evolution as the evolution of biochemical complexity; this is even stated explicitly:

Even now, whenever a new race or variety originates, it is possible to demonstrate that it possesses new biochemical properties and that its metabolism of matter and energy is somewhat different from that of its ancestors. This indicates that some changes must have occurred in the inner structure, that new combinations of substances and enzymes have been formed, that new physical and chemical systems and new relations have been introduced. From our point of view, therefore, the modern process of evolution of living organisms is fundamentally nothing more than the addition of some new links to an endless chain of transformations of matter, a chain the beginning of which extends to the very dawn of existence of our planet.

It is logically necessary to assume that this was once so but the pieces of evolution for which we have evidence, and the changes that we now see taking place, are as often associated with a loss as with a gain in biochemical complexity. Present day organisms are the end product of about 2,000,000



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years of selection and there is no reason to assume that eobionts used preferentially the same elements that organisms use now for most of their activities. Even so, almost half the elements fill occasional biological rôles and the unusual functions are not found only in the most recent species. Thus the animals that use vanadium are an ancient line and a case can be made out for the primitiveness of plants using aluminium and silicon. On present day evidence it is possible that biopoesis involved such colloids as the aluminates and silicates supporting forms of metabolism that are now looked on as freaks. These may on the contrary be relics of originally common metabolic systems that have been ousted by more economical ones based on proteins and phosphoric esters. The commonly made assumption is as baseless as would be the assumption that, because writing now generally involves paper, that was necessarily its original vehicle.

This book is the only comprehensive and authoritative account of the origin of life that has been written during this century. But it is old and recent researches on the conditions in past times have contributed greatly to the problem. A new book is needed and it is to be hoped that Professor Oparin will undertake the task of writing it. If he does so it is also to be hoped that he will not be embarrassed by an Introduction as banal as the one the translator obtrudes into this edition.

N. W. PIRIE

*Leibniz.* By Ruth Lydia Saw.

Penguin Books, Harmondsworth, 1954. Pp. 240. 2s. 6d.

THIS is a very sympathetic and clearly written account of Leibniz's main doctrines. Miss Saw is mainly concerned with his metaphysics, and she invents ingenious models to give greater plausibility to his account of the monads. After a general introduction to his philosophical position, she deals in successive chapters with God, Space and Time, Matter and Motion, and Men, followed by brief accounts of his Moral Theory, his Theory of Knowledge, and his Logic. She is always concerned to relate his views on ultimate reality to our commonsense view of the world, and to show wherever possible how the former can throw light on the latter. Her sympathetic handling does not prevent her from being a cool and detached critic. She has much that is admirable and interesting to say on all her topics, and her treatment is sane and well balanced. She manages to introduce a surprising amount of modern logical and epistemological discussion into her examination of his doctrines.

She does not deal with Leibniz's work in mathematics and dynamics, except in passing, though there is brief reference to his attitude to science in the chapter on Space and Time, Matter and Motion. Since she takes Leibniz to assert that space and time are unreal, she tries to give an account of the states of monads and their relations to one another without

mentioning either space or time. I do not think space and time are 'unreal' for Leibniz in this sense. He himself describes them as orders of co-existence and succession which would be found in any world, and makes no attempt to describe the real world, even from the point of view of God, without mentioning them. Miss Saw sees how difficult it is to get rid of time, but thinks space can be successfully negotiated, and her account (p. 111) of what underlies the appearance of two bodies being near one another in space is an attempt to show how this can be done.

Her solution is based on the suggestion that for one monad to 'approach' another monad is for it to pass from a less clear to a more clear mirroring of it; and the more clearly it mirrors it, the 'nearer' it is to it. This account does not seem consistent with the phenomena of sense perception, especially of vision, whereby higher organisms can be conscious of objects at a distance more clearly than they are of many objects near at hand. It seems to me that there is much more justification in the texts for describing 'nearness' of two monads in terms of relations between their perspectives of the whole phenomenal world: I don't think it can be described without bringing in reference to space, and indeed to phenomena.

Miss Saw adopts a similar attitude toward efficient causes (pp. 112-115). Since she regards efficient causation as belonging only to the phenomenal series, she allows only final causes to figure in her account of the real world. In consequence she says nothing at all about efficient causes considered from God's point of view—a topic of great importance for Leibniz's theory of physics.

Her account of the logic is based on the principle that any complex property can be analysed into simple properties, none of which is incompatible with any other; and this principle makes havoc of what Leibniz wants to say about the world, as Miss Saw shows. I think it is a pity that she follows Russell in describing Leibniz's principle (in the *Discourse on Metaphysics*) that in any true proposition the predicate is in some way contained in the subject, as the principle that every true proposition is analytic, since Leibniz's principle is meant to cover both necessary and contingent propositions, while in the modern sense of the word all analytic propositions are necessary. (Leibniz himself does not apply the word 'analytic' to propositions.) Leibniz's principle requires complex predicates—such at least as are possessed by created beings—to be thought of as somehow not mere collections of simples; and his stress on negation as the source of imperfection and evil and as the ground of the difference between the created world and God also suggests the inadequacy of a logic of positive simples. How far he held to this logic in his later years is not at all clear.

I am not happy about Miss Saw's suggestion (pp. 90, 95) that if Leibniz is to be consistent he must admit continuity not only within the world of created monads but also between this world and God. This is to miss

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what Leibniz would have regarded as the infinite distance between perfection and imperfection, between a necessary and a dependent being. Her problem (pp. 98-99) about whether God has a single point of view from which to look at the universe, or whether he enjoys all points of view, is dealt with, so far as 1686 is concerned, in Section XIV of the *Discourse on Metaphysics*. And I am sure this represents Leibniz's later doctrine.

L. J. RUSSELL

*Aristotle's Philosophy of Mathematics.* By H. G. Apostle.

The University of Chicago Press, 1952. Pp. x + 228. 45s.

THE aim which the author has set himself in this book is to present Aristotle's philosophy of mathematics without going beyond what is found in Aristotle's works or what is implied by them. With this aim in view the author appears to have combed the writings of Aristotle very accurately in search of first-hand material. He has presented his findings in an arrangement which in his opinion might have been adopted with little alteration by Aristotle himself, had he chosen to write a treatise on the nature of mathematics. Thus Chapter I is devoted to a general discussion of mathematics; Chapter II deals with arithmetic; Chapter III is concerned with geometry; in Chapter IV sciences which apply mathematics are discussed; and finally, Chapter V concludes the book with Aristotle's criticism of what other thinkers before him had to say on the nature of mathematics.

The characteristic feature of the author's treatment is that he speaks about mathematics, its principles and its methods, in the language of Aristotle, his own contribution being limited to providing continuity of the presentation. He even finds it convenient to drop the third person singular in favour of the first plural thus blending beyond recognition Aristotle's views with his own occasional remarks. There is undoubtedly something to be said for this kind of approach but in the reviewer's opinion the task of reconstructing Aristotle's philosophy of mathematics cannot be successfully completed without setting his views in a wider historical perspective and without evaluating them in the light of the modern conception of the philosophy of mathematics. Since this has not been attempted by the author, who incidentally ignores the current literature on the subject altogether, his book can hardly have a claim to the status of a critical and appreciative monograph. Nor can it be regarded as an ordered collection of extracts illustrating Aristotle's philosophy of mathematics as, in the process of paraphrasing, the relevant passages have lost their authenticity.

In addition to notes, which have been given the form of references to the Bekkerian edition of Aristotle, the author has compiled an English-Greek dictionary (209 entries) and an index. The dictionary—we are assured in the Preface—has been included and strictly adhered to for the



purpose of retaining definiteness and clarity of Aristotle's terms and meaning. It is supposed to consist of 'important philosophical terms'. The reviewer regrets having found it completely useless and full of irrelevancies. Surely no philosophical interest is served by reminding the reader that 'few' means 'ὀλίγα' and 'many'—'πολλά' or that 'great' means 'μέγας' and 'small'—'μικρός'.

The posthumous commentary by Sir Thomas Heath (*Mathematics in Aristotle*, Oxford, 1949)—which is not mentioned by the author—gives a thorough and reliable analysis of all the most important *loci mathematici* in Aristotle with the emphasis on historical and textual problems. A critical interpretation of the passages from the point of view of the modern philosophy of mathematics followed by an attempt at a cautious synthesis is certainly needed. Unfortunately it is not to be found in the book under review.

CZESŁAW LEJEWSKI

*The Development and Present Status of the Trace Theory of Memory.*

By Bronislaw R. Gomulicki.

*The British Journal of Psychology Monograph Supplements*, XXIX, pp. 85.  
12s. 6d.

THE scope and flexibility of behaviour is intimately dependent on the capacity to store information over periods of time. This is abundantly clear if the higher mammals, including man, are compared with the lower mammals—or, for that matter, if the more impressive calculating machines are compared with the less impressive. Attempts to theorise about memory are thus of considerable interest. In the first three chapters of this monograph, Gomulicki reviews some of the main theories concerning the neural basis of memory which have been held in the past 2,500 years. (It should be explained that he uses the expression 'memory trace' to mean merely 'the as-yet-unknown organic substrate of the memory process', and the title of the monograph should be understood accordingly.) Most of these theories are described too briefly for them to come to life: this makes these chapters rather heavy reading. Nevertheless, in conjunction with the bibliography, they may prove useful for reference purposes. The two remaining chapters, which are concerned with the present position in memory theory, are undoubtedly more valuable. As a general approach, Gomulicki suggests, 'Why not assume, at least until new evidence forces a re-interpretation, that known principles do in fact account for such memory phenomena as in theory they could do; take stock of what remains unexplained on this basis; work out, perhaps by the methods of mathematical biophysics, the requirements of one or more supplementary mechanisms to account not for the whole of memory but only for the otherwise unexplained phenomena; and then concentrate physiological investigations

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on a search for brain mechanisms fulfilling these requirements?' This is sensible, and his attempt to do this is stimulating. Among his specific suggestions, perhaps the most interesting is that there is a double recording of sensory events (i) in the form of an individual trace which has a point to point correspondence with this event, (ii) in the form of a generic trace, which is established after the afferent impulses have been subject to some sort of 'scanning' process by which general features of the pattern are abstracted. The individual trace provides 'the basis for memory of specific events, objects, etc., *as such*, and not simply as members of some general category': the generic trace, for 'generic' memories (which he does not define). However, while dual recording could solve a good many problems in memory theory, this particular dual recording does not seem to be quite what is required. For the individual object or event, as remembered, is always a highly interpreted version of the original stimulus situation, and it therefore seems necessary to suppose that the *individual* trace is only established after the sensory input has been subjected to a considerable amount of scanning. If so, how does it differ from the generic trace? A plausible answer, perhaps, is that the generic trace mediates the acquisition of inductive knowledge of the general properties of the environment—cf. Bartlett's schema. (Maybe this is what Gomulicki had in mind but, if so, he has not succeeded in making himself clear.)

This monograph is ostensibly concerned with the physiology of memory. In fact it contains little reference to physiology, properly so-called, and indeed next to nothing is known about the physiological basis of memory. In its place, there is analogical speculation. What of the future? Is there any hope of a genuinely physiological approach to memory? Gomulicki himself, in an appendix, has two specific suggestions to make, one concerning the possible correlation between excess lipochrome deposits in the cortex and memory defect and another concerning the relationship between 'kappa waves' (detected by an E.E.G.) and the process of recall. However, while interesting correlations of this type may be established, it seems optimistic to expect to gain insight into the detailed functioning of memory via physiology, at least in the foreseeable future. The more promising alternative appears to be the postulation of memory mechanisms having certain general characteristics, followed by an attempt to verify the behavioural consequences to which they would lead. JOHN BROWN

*Science and the Human Imagination.* By Mary B. Hesse.

The S. C. M. Press, London, 1954. Pp. 171. 12s. 6d.

THIS book falls into two parts. The first four chapters are a survey of the history of the physical sciences—in particular, mechanics and astronomy—from Greek times until the twentieth century. The remaining chapters are a critical commentary on some philosophical interpretations of scientific

theories. The link between the historical and the logical parts of the book lies in Dr Hesse's apologetic purpose. She wants to show that science and religion (that is, the Christian religion) can each benefit by a *rapprochement* and an abandonment of the suspicious neutrality that has existed between them since Victorian times. To this end she tries to establish two main points. In the historical part of the book, she aims at showing that an understanding of the historical background of the scientific revolution of the seventeenth century will reveal that the scientists themselves were indebted to the Christian tradition of the west for their new view of the world. Thus the scientists' approach to nature must be regarded as a fulfilment of the Christian attitude rather than a rejection of it. Secondly, she examines critically some positivist and operationalist accounts of scientific theorising in order to bring out what she considers to be their inadequacies. She then proposes in their stead a view of scientific thinking as *analogical*. If we are prepared to regard scientific theories in this light, the supposed gap between science and religion will have been narrowed. Since it is well known that theologians have usually claimed that our knowledge of God is analogical, the recognition that scientific knowledge is also of this kind will bring out the essential similarities between knowledge of nature and knowledge of God and so 'contribute to a lessening of the tension between the scientific and the Christian attitudes to the world'. Thus Dr Hesse claims that scientific ways of thinking have grown out of Christian ways of thinking and are still, if rightly interpreted, essentially similar to them.

If I have not misunderstood her main argument, she is making a large claim. Indeed I do not think that she has allowed herself, in this short book, sufficient space to state and defend it adequately; and perhaps most of the defects of the book can be traced to this source. Her historical contention, which is the more original of the two, comes off worse in this respect. For it would take a very detailed study of the thought of the late Middle Ages and of the sixteenth and seventeenth centuries to make this very surprising claim plausible. No doubt it is true that some seventeenth century scientists invoked, in their war with the Aristotelians, 'the medieval view of nature as a sacrament of the divine creator and of science as a servant of greater ends than itself'. But this might well have been a convenient *argumentum ad hominem* against a fellow Christian or even a rationalisation of a scientist's purely secular motives. Such matters are notoriously difficult to establish even by very extensive historical enquiries. Though I do not wish to suggest that Dr Hesse's thesis is false, it is certainly quite unproven. It is, however, an interesting suggestion that historians of science would do well to explore.

I feel a good deal less happy about her methodological thesis. And this, it seems to me, is the more important of the two. Whatever the defects may be of a purely descriptive or a purely operationalist account of



scientific theories—and Dr Hesse does point to some of their weaknesses—her doctrine of scientific theories as analogical is, at least in the cursory form in which she has to present it, far too vague even to rank as an alternative. In so far as an analogical account of scientific theories intends to say more than is conveyed in the familiar notion of an explanatory model, it is hopelessly indeterminate. Nor does it help here to invoke the medieval doctrine of the analogy of being. For there was no such unitary doctrine. An examination of any of the historical studies of this concept (for example, Lyttkens' recent study of Aquinas' view of analogy) will show what an extraordinary medley of semantic confusions this so-called 'doctrine' consisted of.

Dr Hesse seems to me to be unwise in her anxiety to disturb the foundation of the tacit non-interference pact that has been observed between science and religion during the present century. I cannot see what gain to religion she expects from this or even how her desired *rapprochement* could possibly be brought about. After all, it can be shown very easily that there is no *incompatibility* between Christian beliefs and scientific knowledge by the bare fact that many scientists are professed Christians. As long as it is admitted by theologians that religious statements do not make the same kind of logical claim as scientific ones, there is no need for the believer to keep his scientific and his theological beliefs in separate mental compartments. For the contentions of modern theologians, unlike those of their unwary predecessors, tend to be so formulated as to be invulnerable to any sort of scientific refutation. They therefore stand in no danger of conflicting with the results of science and, for the same reason, they are logically incapable of receiving any sort of support from them. The penalty of invoking the support of scientific evidence for religious doctrines is to invite, at the same time, their refutation. The theologian cannot have it both ways and on his past experience he has much more to fear from the scientist than to hope for from him. But in order that empirical evidence can be even relevant to religious statements, these statements have first to be given some sort of empirical content. And, as Dr Hesse admits in her epilogue, it is not easy to see how this can be done. Moreover, if religious statements are admitted to lack empirical content, it is extraordinarily difficult to see what sort of content they *can* claim. And, it is this difficult question of the semantic status of religious propositions that must first be made clear if anyone wishes to explore the relations between science and Christianity.

I have noticed one or two slips and misprints. It is inaccurate to refer (p. 34) to the 'atheistic commentaries' of Averrhoes. The late Sir Thomas Heath appears on p. 30 as 'Professor T. E. Heath'. The most serious misprint is in the relativity mass equation,

$$m = m_0(1 - v^2/c^2)^{-\frac{1}{2}},$$

which appears on p. 73 without the index  $-\frac{1}{2}$ .

D. J. O'CONNOR

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*Das Alter des Universums.* By Joseph Meurers.

Westkulturverlag Anton Hain, Meisenheim am Glan, 1954. Pp. 103.

Brosch DM. 6.70, Leinen DM. 9.

THIS monograph is concerned with metaphysical aspects of the statement that the age of the Universe is finite. The scientific reasons from which an age of the order of  $10^{10}$  years is concluded are briefly presented. It is pointed out that these conclusions are based on the assumption that the laws that govern the behaviour of the universe are universal, while at the same time this assumption is contradicted by the implication that their universality is restricted in time. At least that seems to be the argument expressed by the author when he speaks of a contradiction inherent in a definite age of the universe. But the argument, though repeated many times, is never expressed very cogently. The history of the concept 'time' is reviewed since the days of Ancient Greece and a distinction made between endless time, which implies an infinite succession of events, and eternity which implies absence of events. It is pointed out that a scientific statement only has meaning if it applies to a measurable quantity; and this must have structure. So a statement about structureless time has no scientific meaning. The implication seems to be that the beginning of the universe must be equated with the beginning of time. The extrapolation is assumed to go back to an initial condition where there was equilibrium, a condition previous to the world as we know it. Such a condition is, by definition, eventless and therefore timeless. Extrapolation cannot by its nature give any information about the process of the creation.

On the relation between the concept of time and the assumption of a finite age of the universe, the author has little to say that is not expressed more clearly, more briefly, and more cogently by Mr Michael Scriven in this *Journal* in November 1954. But Mr Meurers departs from that comparative orthodoxy in his later chapters where he introduces the hypothesis of a cosmic entelechy. By steps of reasoning that are not very apparent he argues that this concept is necessary in order to resolve the above mentioned contradiction. He defines the cosmic entelechy in one place as a tendency inherent in the structure of the universe, not deducible (*denknotwendig*) from it, not affecting causal laws, and determining successive conditions for the universe. In another place the entelechy is defined not as a tendency but as an effective influence. The assumption that one term is as good as another is unfortunately not peculiar to this one author. The philosophy of emergence, which was very fashionable earlier in this century, but has few supporters at the present time, was rendered untenable by the same confusion. Qualities of which one can only say what they are like were equated with agents of which one can only say how they act. The plausibility of that doctrine depended on ignoring the difference between the verb to be and the verb to do. It is surprising to meet a revival at the



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present time of a view so near the one expressed by the Emergent school, which was, that God or an entelechy emerges from the relationship between the objects that compose the material universe.

The monograph is difficult to read and the chief reason for this is its repetitiveness. The same statements recur over and over again, sometimes in different and sometimes in nearly the same contexts; sometimes with identical and sometimes with slightly varied wording. The reader has on each occasion to ponder whether something new is being said or whether the slight changes have no significance. It is worth while to draw attention to this defect because it is met with in philosophy with other authors, though rarely in so extreme a form. It is a lazy habit to hope that repetition of a statement will serve the function that can only be served when an author takes the immense trouble of finding for the statement the most precise, the most cogent, the most memorable formulation together with the one and only most functional setting.

It is an interesting and perhaps significant feature of Mr Meurer's work that he does not mention the name of a single non-German writer except some Ancient Greeks and St Thomas Aquinas. Such isolation from the thought of all those philosophers and scientists who have had profound things to say about the nature of time, the age of the universe, the problems of organisation and structure and similar subjects discussed by Mr Meurer is indeed striking. His way of discussing such subjects does not reflect any familiarity at all with the work done outside his own country. If the author had been more conversant with thought in other countries he would have been saved from groping his way through regions already fairly well charted, and might have turned his undoubtedly acute reasoning powers to more rewarding use. May it be that we see here the consequences of the isolation from cultural thought so effectively imposed on German academic circles during a decade and a half not so long ago. R. O. KAPP

*A Synoptic Index to the Proceedings of the Aristotelian Society.*

Edited by J. W. Scott.

Basil Blackwell, Oxford, 1954. Pp. x + 0206 + 127. £3

THIS is a valuable book of reference. It contains synopses of all papers published in the *Proceedings of the Aristotelian Society* (including Supplementary Volumes) from 1900 to 1949. It is arranged in two parts, alphabetically under authors and subjects, with suitable cross references. The number of papers on philosophy of science is small. J. O. WISDOM

### *Corrigendum*

In 'Two Autonomous Axiom Systems for the Calculus of Probabilities', by K. R. Popper, 1955, 6, No. 21, p. 57, 3rd line of last paragraph: insert '(and that the limit exists)', after 'definition 4'.



## RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

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- Ivan D. London, 'Quantum Biology and Psychology', *The Journal of General Psychology*, 1952, **46**, 123
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- Frederick L. Will, 'The Justification of Theories', *The Philosophical Review*, 1955, **64**, 370
- F. E. Zeuner, 'Absolute Zeitrechnung', *Studium Generale*, 1955, **8**, 479